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ESSAYS IN CHILD WELFARE

by

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A DISSERTATION

Presented to the Department of Economics
and the Graduate School of the University of Oregon
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Title: Essays in Child Welfare

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DISSERTATION ABSTRACT

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Title: Essays in Child Welfare

We empirically explore the effect of attorney representation for foster care youth in abuse and neglect hearings on adoption outcomes. By exploiting temporal variation in the passage of state-level statutes that mandate all foster children have the right to representation, we find that child attorneys not only expedite the adoption process, but also improve upon the stability of these adoption matches. When exploring the determinants of child maltreatment, we exploit variation in the timing of state-level medical marijuana legalization to identify the effect of caretaker marijuana use on reported and substantiated rates of child maltreatment. As medical marijuana laws increase general population marijuana use, we find that treatment leads to significant reductions in the rates of reported and actual perpetration of child physical abuse, with no simultaneous increases in the rates of alternative forms of maltreatment perpetration. We next examine a policy related to alternative child well-being measures. Within a difference-in-differences

framework, we consider the effect of antibullying laws (ABLs) on adolescent mental health outcomes. We find that overall, laws with an implementation deadline result in modest reductions in adolescent suicidality and illicit drug use. However, when considering the effect of heterogeneity across ABL attributes, we find that more costly procedural components lead to increases in reported suicidalilty and drug use, suggesting the implementation of such components may displace existing mental health resources.

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To my family.

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CHAPTER I

INTRODUCTION

Numerous public agencies throughout the U.S. make up the child welfare system which is designed to protect one of our most vulnerable populations. Through applied econometric analysis, this work aims to inform and shape effective child welfare policy as it relates to disadvantaged and maltreated youth.

In Chapter II we consider the effect of legal representation for foster youth on the probability of adoption. Exploiting temporal variation in state-level statutes that mandate that a foster child has legal representation in dependency proceedings, we find that mandated representation induces earlier adoptions, with the probability of adoption within two years of entry into foster care increasing 13 percent on average. We observe larger impacts for children most at-risk of aging out of the foster care system. Conditional on adoption, legal counsel also leads to a decrease in the probability of short-term foster care reentry. We find no evidence that children are any more likely to be adopted due to representation.

In Chapter III, using the passage of state-level medical marijuana laws, we empirically study the effects of marijuana use by analyzing data on child maltreatment reports in the U.S. We find that caretaker marijuana use decreases the rate of child physical abuse by 9%, with no effect on the rate of child neglect. Differential effects suggest the impact of marijuana use on the perpetration of child

physical abuse is driven by males, and is greater for white caretakers, and those over the age of 25

Finally, in Chapter IV, we examine how state antibullying laws influence bullying related mental health outcomes—specifically, we focus on teen suicidalty and substance use. Using data from the Youth and Risk Behavior Surveys, and the Multiple Cause-of-Death files, we find that laws with a clear implementation deadline are associated with 11% reductions in teen sucidality and a 13%-22% reduction in drug use. However, we also find suggestive evidence that existing mental health resources in schools may be being displaced, as the more-expensive components of antibullying laws are associated with increases the probability of illicit drug use and suicide.

CHAPTER II

ADOPTION OUTCOMES FOR FOSTER CARE CHILDREN: THE ROLE OF ATTORNEY REPRESENTATION

Introduction

Over the last decade, an annual average of over 400,000 children are left in the foster care system—also referred to as out-of-home care. Of the children left at the end of the year, only about 250,000 will discharge from the system in the following year, with 20,000 of those children aging out (AFCARS Report 2005-2015).[1] Not only do the vast majority of foster children come from disadvantaged backgrounds, but also entry into the foster care system leads to a plethora of negative health and human outcomes. Doyle Jr (2007) and Doyle Jr (2008) exploit the random assignment of lenient caseworkers to families with an instrumental variable approach, and find that foster care placement increases teen pregnancy, unemployment, juvenile delinquency, and delinquency in adulthood. Other studies have linked foster care entry to mental health and behavioral problems (Blome, 1997; Newton et al., 2000; Rubin et al., 2004, 2007; Pilowsky and Wu, 2006), illicit drug use (Pilowsky and Wu, 2006), high-school incompletion (Blome, 1997; Pecora et al., 2006), and homelessness (Pecora et al., 2006). Still others have suggested that the link between foster care entry and negative health and human-capital

[1] "Aging out" of the system occurs when a foster child reaches the age of legal adulthood and is then emancipated from the foster care system.

outcomes is increasing in the time spent in the system, and for children who never discharge to a stable environment (Pecora et al., 2006; Rubin et al., 2007).

The potential welfare improvements associated with effectively facilitating expedient and stable permanency outcomes for foster children are significant. In this paper, we explore the effect of mandated legal counsel for children in out-of-home care—this type of counsel is referred to as a lawyer-guardian *ad litem* (LGAL)—on adoption outcomes. Many researchers and child-welfare experts consider adoption the judicious outcome for many foster children (Triseliotis and Hill, 1990; Triseliotis, 2002; Van Ijzendoorn et al., 2005; Vinnerljung and Hjern, 2011) and, yet, very little is known about the effect of legal representation on adoption outcomes. When a child is removed from a home, a "case plan" details the plan for how that child will ultimately discharge from the system, and is determined in a series of dependency court proceedings.[2] LGAL representation is motivated out of an expectation that legal training uniquely offers resources that allow one to more-effectively negotiate the dependency court process, where any determinations made shape outcomes for children and families.

Prior research on legal representation for foster youth suggests that representation is correlated with reduced time in the foster care system and increased adoption rates. Here, we exploit state-level variation in the timing of legislation that mandates that all children in the state's foster care system have the

[2]Dependency court hears about minors who are abused, neglected, or otherwise reside in an unsuitable home environment.

right to legal representation during dependency proceedings, to retrieve an estimate of the causal parameter of interest—the effect of LGAL mandate on adoptive outcomes. In a difference-in-differences framework, we consider whether children entering foster care in a mandated-LGAL regime are more or less likely to discharge to an adoptive home.

Currently, there are 15 states without a statute in place, and in some of these states as few as 8% of foster children receive legal counsel in neglect and abuse hearings. Since LGALs are vital to informing the court of both the facts and the child's preferences over placement options, many legal- and child-welfare experts advocate for the right of all foster children to receive legal counsel in dependency hearings.[3] Therefore, as this right becomes increasingly common, it is crucial to understand how lawyers shape foster-youth permanency outcomes, and if mandated representation plays a productive role in improving the welfare of this vulnerable population.

We find that mandated LGAL representation increases the probability of adoption in the first-two years of foster-care tenure—roughly 30 additional children in the average LGAL state find adoptive homes within two years of entry than in control states. Results suggest that this increase is primarily driven by expediency in the adoption process, and not by changes in adoptive outcomes on the extensive margin or in reunification and guardianship outcomes. (The average foster child

[3]Source: Quality Improvement Center on the Representation of Children in the Child Welfare System: Needs Assessment Literature 2010, U.S. Department of Health and Human Services.

waiting to be adopted spends almost three year in the system.) In addition, we find evidence that even children who enter the foster care system prior to the law still experience more expedient adoptions when appointed legal counsel after-the-fact. Moreover, given these partial treatment effects, the results from our event study analysis alleviate concern that the passage of state laws is related to relevant unobservable state characteristics, and as such we will be inclined to attribute these difference in foster child outcomes associated with the LGAL representation as causal.

We also investigate how these impacts differ across subgroups of the foster-care population, with particular interest in considering any systematic change to the paths to adoption among children who are at greater risk of aging out of the system without a permanent home (e.g., older children, racial minorities, those who have suffered abuse). Indeed, we find larger increases in rates of two-year adoptions among adolescents and abused children.

Last, to consider the stability of adoption matches we ask whether LGAL mandates are coincident with changes in the probability of foster care reentry from an adoptive home. We find that the probability of reentry within two years significantly decreases with LGAL, which suggests that child attorneys not only expedite the permanency process, but do so with no perceivable cost to the quality of adoption matches. Our results provide compelling evidence that LGALs increase the welfare of foster care children. We thereby inform states that are contemplating

the passage or removal of LGAL-like legislation, and federal policymakers who control the minimum standards within dependency court proceedings.

In Section 2, we discuss the adoption process in more detail, as well as consider the related literature, background, and context for considering the policy changes we exploit for identification. In Section 3, we describe the data sources, and, in Section 4, we develop our empirical specification. In Section 5, we establish the impact of LGAL mandates on the probability of adoption, length of time to adoption, and discuss competing placement outcomes. Before concluding, in Section 6, we consider the heterogeneous effects of LGAL representation across different foster care populations as well as the effect of legal representation on foster care reentry.

Background

Foster Care and Adoption

Legal counsel for foster children plays a primary role in determining a foster child's eventual exit from the system.

In Figure 1, we consider the typical process through which children enter and exit the foster care system. The average number of children below the age of 15 entering the foster care system (for the first time) each year is close to 122,000. From entry, we map children through to discharging from foster care within four

years, by type of discharge.[4] Within the first four years of foster care tenure, about 53% of foster children will reunify with their families. If reunification is not possible, 17% of foster children will discharge to a legal guardian, and in the case parental rights are terminated, 22% will discharge to an adoptive home.[5] Reunification, adoption, and legal guardianship constitute permanency placements (U.S. DHHS, n.d.), and if a foster child does not attain permanency they generally age out of the foster care system at age 18.[6] Of first time entrants below the age of 15, about 2% will age out within 4 years. The remaining 6% of foster care entrants will stay in the system for more than four years.

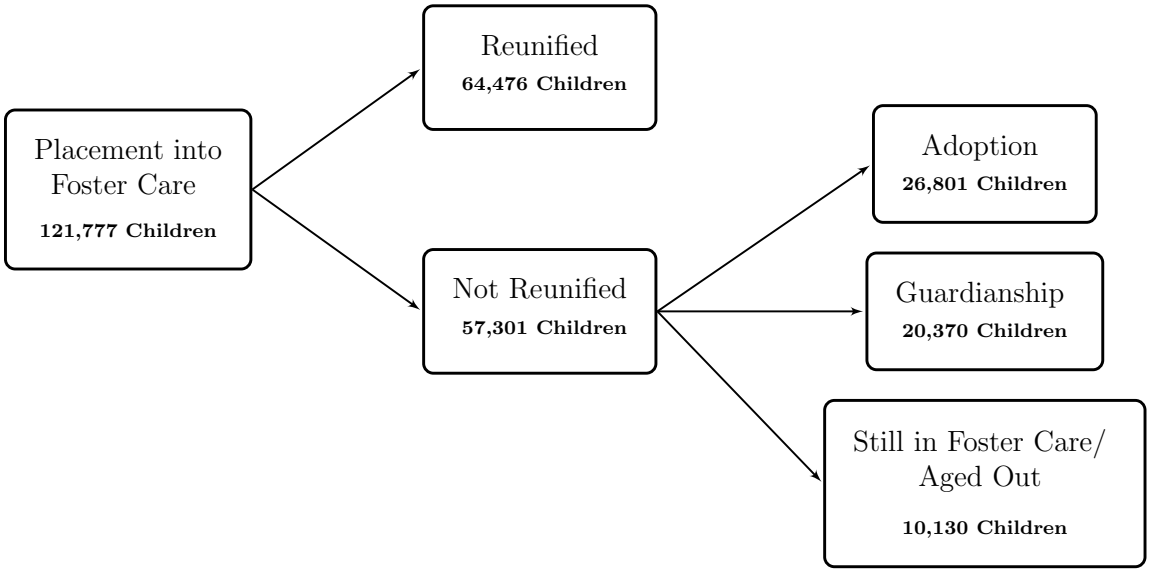
Of the children who are ever adopted in the sample between 2001 to 2014, 88% (262,879) are adopted within their first four years in the foster care system (Figure 2, Panel A), and of those adopted in the first four years the probability of exit to an adoptive home is highest in the second and third year upon entry (Figure 2, Panel B), with respective probabilities of 0.30 and 0.35. Many studies have found age to be a significant predictor of foster care exits, with older children less likely to attain any permanent outcomes and infants more likely to be adopted (Barth, 1997; Connell et al., 2006; McDonald et al., 2007; Akin, 2011). In our sample, the probability of discharge to an adoptive home for children below the age of 5 is 0.33,

[4]Children who exit foster care because they died, were transferred to another agency, or ran away are excluded from our study, altogether this group accounts for less than 1% of the sample.

[5]Of the children for whom parental rights have been terminated, 93% will exit to an adoptive home, and about 2% will exit to a legal guardian. The majority of guardianship exits occur when parental rights are still in tact.

[6]Note, in several states the oldest age at which a child can stay in the foster care system is over 18, however the minimum age nationwide is 18 years old.

FIGURE 1.
Entry to Discharge in the Foster Care System (Within 4 Years)



Notes: The data source is AFCARS Foster Care files, 2001-2014.The sample is restricted to first episode foster-care children aged 14 and below, who entered the system by September 2010. The figure summarizes the annual average number of children who enter and will exit foster care within 4 years, by category of exit.

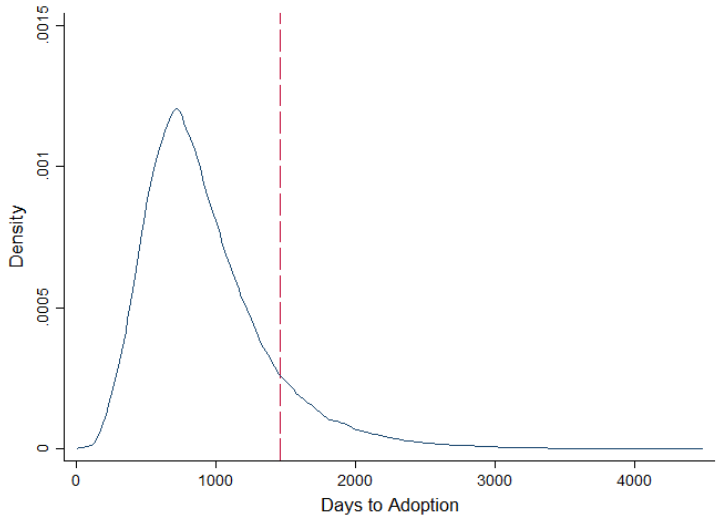
for those aged 6 to 10 years old that probability is 0.15, and for adolescents above the age of 11 the probability of adoption is 0.07. Studies have generally shown that racial minorities and children with a history of abuse are less likely to be adopted, and face a longer time to adoption (Courtney and Wong, 1996; Barth, 1997; Potter and Klein-Rothschild, 2002; Connell et al., 2006; McDonald et al., 2007; Akin, 2011). In our sample, 25% of black children will be adopted (versus 45% of white children) and only 18% of abused children will be adopted. For both subgroups, the average length of time to adoption is above the sample average of 2.6 years. Some have argued that legal counsel for foster youth may have larger impact on adoption outcomes for historically disadvantaged foster care populations. Thus,

after examining the overall effect of legal counsel on adoption outcomes, we explore the potential role of attorney representation in alleviating adoption discrepancies across foster care populations.

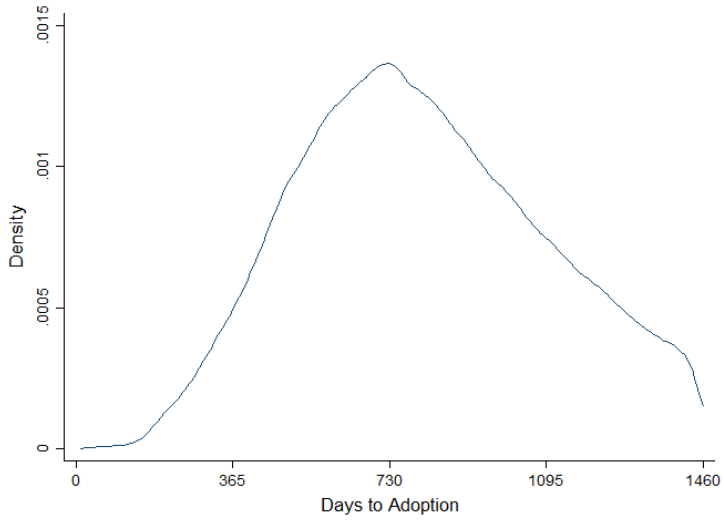
Previous analysis of the impact of foster care policies on adoption outcomes have primarily focused on financial incentive programs, such as adoption subsidies and federal matching grants. Exploiting state-level variation in the age cutoff for special needs designation required for federal adoption subsidies, Buckles (2013) finds that subsidy eligibility decreases time spent in foster care for children who are adopted. Within a difference-in-differences framework, Brehm (2018) considers the effects of the federal adoptions incentive program whereby states receive payment for every adoption of a child above the age of nine. She finds that increasing these financial incentives did not increase the probability of adoption for older children relative to younger children, and similarly did not affect the timing to adoption. Exploiting discontinuities in foster care subsidy payments by age, Argys and Duncan (2013) find that the smaller the value of adoption subsidy payments relative to foster care subsidy payments, the smaller the likelihood of adoption. Relatively little has been done to explore the impact of alternative policies and court reforms on adoption outcomes, despite the ubiquity and significant costs associated with such programs.

FIGURE 2.
Days from Entry to Adoption, Kernel Density Estimate

(a) Days to adoption ever



(b) Days to adoption within 4 years



Notes: The data sources for the figures is AFCARS Foster Care Files, 2001-2014. Both panels (a) and (b) are calculated using the Epanechnikov kernel with a bandwidth of 26.5. The red dashed line in Panel (a) corresponds to discharge (to an adoptive home) in year 4 of foster care tenure.

Lawyer-Guardian Ad Litem

When a child is removed from their home and placed into the foster care system, a dependency petition is filed and a series of trials and hearings occur as part of the judicial process. It is in these hearings that a judge determines whether the current home environment is unsuitable, and, if so, the child's permanency plan.[7] In Figure 3, the timeline for major milestones of these dependency proceedings is captured. Every foster child's case, regardless of reason for system entry, must be reviewed at least once within the first six months in out-of-home care—at the six-month review hearing, the court may decide to return the child to their home or order the child to stay in foster care.[8] In addition, a permanency planning hearing must be held within the first 12 months of a child's placement in out-of-home care. At this hearing, the court will select the child's permanency plan—whether efforts to reunify the family should continue, or if not (or termination of parental rights has already occurred) a plan for adoption or legal guardianship.[9].

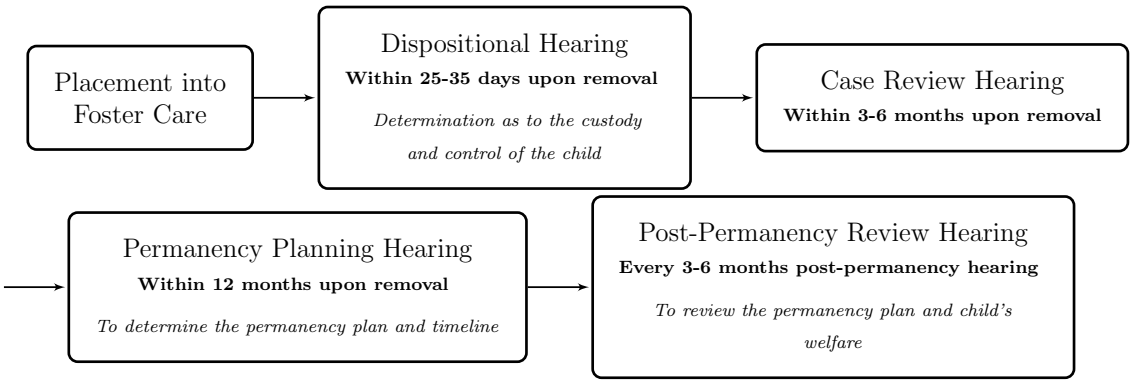
If a foster child is represented by a lawyer-guardian ad litem—a guardian ad litem (GAL) who is a licensed attorney—throughout the dependency proceedings, this lawyer will play a crucial role in determining and facilitating the child's

[7]A dependency petition is a court document filed by an interested party, usually a child case worker or law enforcement official, concerned about the welfare of the child.

[8]For children who are placed in foster care for reasons other than maltreatment, such as parents' incarceration or parents' inability to provide special needs care, the six month review hearing may be the child's first encounter with the juvenile court process.

[9] Adoption and Safe Family Act (42 U.S.C. 675(5)(B) (2015), 42 U.S.C.A. 675(5)(C) (2015))

FIGURE 3.
Legal Milestones In Dependency Proceedings



Notes: There is state variation in the exact hearings and child-protection-case timelines; this highlights a few major dependency trails and hearings as well as the maximum length of time to each milestone as mandated by the Adoption and Safe Family Act (42 U.S.C. 675(5)(B) (2015), 42 U.S.C.A. 675(5)(C) (2015))

permanency plan.[10] Most support for child representation comes from the necessity of legal expertise and resources for navigating through the complexities of the dependency court process (Duquette and Darwall, 2012). In addition, lawyers may better ensure the child's interests are represented in court by way of better informing and communicating to their child clients the permanency outcome possibilities and probabilities (Gueinzius and Hillel, 2014; Elrod, 2006). Last, lawyers will arguably ensure the court decision making process is less subjective, and consequently less influenced by bias and stereotype which could ultimately help alleviate permanency disparities across age, race, and socio-economic status (Hartmann, 1997; Gueinzius and Hillel, 2014).

[10]A GAL is an individual appointed by the court to investigate what solutions would be in the "best interests of a child."

Although the general consensus among researchers is that children in dependency hearings require legal representation, there are a few critiques of this stipulation. Most criticism of the necessity of legal counsel is expressed as a doubt that lawyers can distinguish between legitimate safety cases and those in which the child is not in danger; or others simply argue that a lawyer may not always be appropriate for addressing matters of complicated inter-personal relationships (Brooks, 1996; Weinstein, 1997; Guggenheim, 2005). Appell (2008) argues that it is difficult to appreciate the arguments posed by either side, because "the lack of research about the effectiveness of (dependency) attorneys leave the value of attorney representation unclear."

To our knowledge, only two empirical studies have previously explored the impact of attorney representation for foster youth on permanency outcomes. Slowriver and Zinn (2008) explores the differences in permanency outcomes between a group of foster children appointed legal counsel and a control group of foster children in Palm Beach County, Florida—treated children exit to adoptive homes faster than children in the control group, with the strongest association among white children and children between the ages of 1 and 7 years old. However, these results potentially suffer from bias due to selection into the treatment and control group.

Orlebeke et al. (2016), exploiting a randomized control trial conducted in Georgia and Washington, finds that children assigned to attorneys who received

additional training (ie, "treatment" lawyers) were more likely to attain permanency within the first six months compared to children who were represented by control attorneys. However, there is no difference in the likelihood of permanency between the two groups for children assigned to lawyers after at least six months in care. Although Orlebeke et al. (2016) does not speak to the general efficacy of lawyers (but rather the efficacy of a specific training program), the results lend suggestive evidence of a possible link between between legal representation for foster children and time spent in foster care. Importantly, note that the assignment of a child to any lawyer was not randomized, and thus, the results of this study do not speak to the general foster care population but only to those who were selected into attorney representation.

Although no other empirical studies have examined programs providing legal representation for foster children in dependency hearings, some studies have explored the impact of alternative court improvement efforts on permanency outcomes. Courtney and Blakey (2003), also exploiting a randomized control trial, finds that cases assigned to an accelerated permanency review process were associated with faster terminations of parental rights (TPR) and adoption than cases assigned to the standard case review schedule.[11] Festinger and Pratt (2002) finds that in post-TPR cases heard by a single judge, adoption was finalized faster,

[11]The accelerated permanency review process mandates that case review hearings occur more frequently than every six months. One limitation of the study is an inability to distinguish between those who experienced TPR but were still in foster care versus those who exited to adoption.

as opposed to cases where there was no judge continuity. However, these same cases received additional case management services making it difficult to isolate the specific effect of any one aspect of the program. Due to the aforementioned limitations of previous studies it is hard to causally interpret the effect of dependency court reforms on adoption outcomes. Our study is the first to exploit plausibly exogenous variation in the timing of state-level LGAL mandates to estimate the casual effect of LGAL representation on adoption outcomes.

Policy Background

In 1974, Congress passed the Child Abuse and Neglect Prevention and Treatment Act (CAPTA), which tied eligibility to federal grants on the provision that states provide representation in the form of a guardian ad litem for children in dependency proceedings (Duquette and Darwall, 2012). This was the first time the issue of a child's right to any form of independent representation in dependency court was addressed in federal legislation. Although a later amendment of CAPTA mentions that a GAL can be a licensed attorney, as of present CAPTA does not mandate states provide independent legal counsel for children in dependency proceedings. Nonetheless, shortly after CAPTA first passed, states began to draft independent legislation mandating that children in dependency hearings must

receive a GAL that is a licensed lawyer; at present, there are 34 states with a mandatory LGAL statute in effect.[12]

TABLE 1.
States that Passed Mandated Lawyer-Guardian ad Litem Statute Between 2001 and 2013

State	First Year	Last Year	Treatment Date	Years of Treatment
OH	2001	2014	9/2003	10
MT	2001	2014	7/2006-12/2011	4
TN	2004	2014	7/2007	6
MO	2001	2014	9/2008	5
RI	2001	2014	7/2009	4

Notes: South Carolina had an LGAL mandate in effect from July 2008 to July 2010, but is removed from our primary analysis.

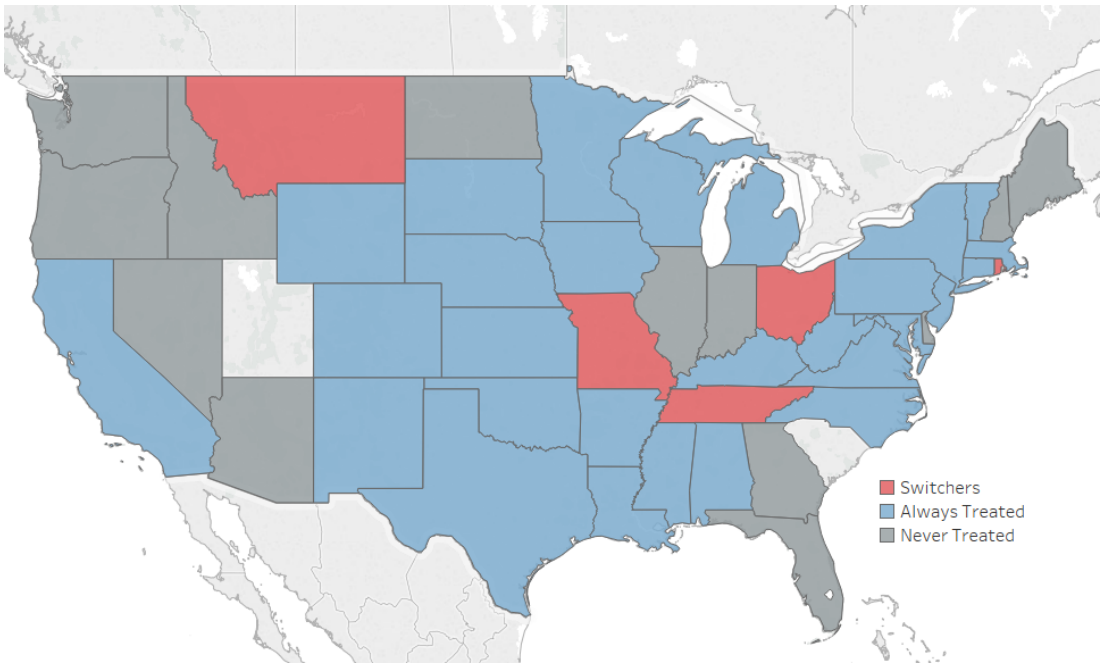
In our sample of foster care entries from 2001-2014, five states passed legislation mandating LGAL counsel for foster children—Ohio, Montana, Tennessee, Missouri, and Rhode Island—the effective dates for LGAL legislation in these states are summarized in Table 1.[13] We exploit the variation in the timing of legislation in these 5 states for identification. During this same time period, 31 states always have an LGAL mandate in effect, and 15 states never have a mandated LGAL legislation in effect—for more details, refer to Figure 4.

In states where LGAL representation is not mandatory, foster children can still be represented by an attorney in dependency hearings. The data does not

[12]The majority of states with a mandated LGAL statute initially passed the legislation in the late 70s and the 80s.

[13]To the best of our knowledge, Utah has passed a mandatory LGAL mandate, however the effective date of the legislation could not be confirmed, therefore Utah has been dropped from further analysis. In addition, South Carolina passed an LGAL legislative mandate in 2008 but revoked it in 2010. Therefore it is dropped from further analysis. When it is included as a treatment state, and we replicate the analysis conducted in tables 3 and 4, we find estimates that are similar in magnitude and statistical significance.

FIGURE 4.
State Mandated Lawyer-Guardian ad Litem Statutes, 2001-2014



Notes: States labeled as "Switchers" enacted a mandate during the sample period (2001-2014). States labeled "Always Treated" and "Never Treated" respectively always had a mandate in effect and never had a mandate in effect, during the sample period. Hawaii and Alaska, not indicated in the figure both never had a mandate in effect. South Carolina and Utah are left unlabeled as data from these states are dropped from the study.

allow us to identify whether a child is or is not represented by an attorney, but in the average non-mandating state, less than 30% of foster care children are represented by an attorney at any given time, with some states providing legal counsel to as low as 8% of foster care children.[14] Note that there is considerable heterogeneity in legal counsel appointment both across and within states; on average, courts in counties with greater access to resources and funds are

[14]Note, anecdotal evidence suggests that, prior to the passage of the mandate, our five mandate-passing states, on average, provided legal counsel to a higher-than-average proportion of foster care children. However, due to data limitations we can not determine the exact probability of receiving legal representation pre-law.

generally most likely to provide foster child attorneys in the absence of a mandate. Furthermore, in the absence of an LGAL mandate, children are most likely to receive legal representation in dependency court if they are above the age of 12-to-14, if the child's expressed wishes oppose the recommendation of their non-attorney representation, when parental rights are terminated, if they have been in out-of-home care for an extended period of time, or if the case is deemed "complex."[15]

Although it is hard to judge which foster children "are most in need" of counsel, it appears that in the absence of a mandate, lawyers are generally appointed to cases where they are likely to have some of the largest impacts (such as cases pertaining to teenagers, and cases where the expressed best wishes of the child conflict with the stated best interests of the child). Furthermore, in our subgroup analysis, we see that those children who were most likely to have received counsel prior to the mandate—such as adolescents—are actually those who appear to still benefit the most with the passage of the law suggesting there is not a large, if at all, quantity-quality trade off with the passage of such statutes.[16] Given the above discussion, we likely find lower bound estimates for the effect of LGAL representation on the probability of adoption.

[15]Sources: (Orlebeke et al., 2016); A Child's Right to Counsel (2007, 2009, 2012), First Star; Seattle Times, 2016; `https://www.law.ufl.edu/_pdf/academics/centers-clinics/centers/legal-rep-of-dep-children-12.pdf`

[16]Anecdotal evidence from state dependency court office representatives also suggests that an LGAL mandate did not result in lower quality legal representation for foster youth.

Data

In the consideration of how lawyers for foster care children impact the probability of and time to adoption, our data source for foster-care enrollment are the Foster Care files of the Adoption and Foster Care Analysis and Reporting System (AFCARS) from the federal fiscal years (FFY) 2001-2014.[17] AFCARS is a federally mandated administrative database; data are reported by states to the Children's Bureau of the Administration on Children, Youth, and Families. The AFCARS data contain detailed case information on the universe of children who are placed in foster care through state welfare agencies; it contains child-level information on race, gender, ethnicity, and age at removal. The AFCARS data also include detailed case histories, including the date and reason of the child's most-recent removal from home, and the date of discharge and discharge type. Each observation in the dataset is an entry into the foster care system, and since we use the sample of first-time foster-care episodes, each child contributes to only one observation. Since our primary outcome of interest is the probability of adoption in each of the first four years upon entry, we additionally restrict the sample to those aged 14 and under to take into account aging out of the foster care system at age 18—this restriction allows the sample of adoption-eligible children to be consistent across the first four years.[18] Similarly, since we can only observe discharges until

[17]The AFCARS Foster Care files are distributed by the National Data Archive on Child Abuse and Neglect (NDACAN).

[18]85% of first-time foster care entrants are below the age of 15.

the close of the 2014 FFY, we restrict the sample to those who entered prior to October 2010.

In Table 2, we present summary statistics for the study sample of children in foster care. As previously mentioned, about 53% of the sample will be reunified, 25% will be adopted, 17% will discharge to a legal guardian, and about 1.8% have not yet discharged from the foster care system. Reentry into the foster care system, within two years, from an adoptive home occurs for 0.3% of the sample; generally, reentry from an adoptive home is a relatively rare outcome. 75% of observations listed maltreatment as a reason for home removal, and of those, 31% specifically listed sexual or physical abuse as a reason for home removal.[19] White and black children respectively make up 45% and 25% of the sample. Hispanics comprise 21% of the sample, and females comprise 49% of the sample. The average child entering foster care for the first time is five years old at entry, and will spend 1.6 years in the foster care system. Detailed child and case characteristics will allow us to explore how legal representation differentially affects adoption outcomes across subgroups of the population, such as age, race, and gender.

We obtain state-year population data from the National Cancer Institutes's Surveillance Epidemiology and End Results (Cancer-SEER) program. State-year median household income and state-month unemployment rate data come from the Bureau of Labor Statistics. To control for state-year welfare expenditures I obtain

[19]AFCARS allows for multiple reasons for home removal to be recorded, a child is classified as "maltreated" if at least one of the reasons for home removal include neglect, physical abuse, or sexual abuse.

TABLE 2.
Descriptive Statistics of Children in Foster Care, AFCARS 2001-2014

	Entire Sample		**Control**		**Treatment**			
					Pre-Treatment		Post-Treatment	
Variable	Mean (SD)	Obs.	Mean (SD)	Obs.	Mean (SD)	Obs.	Mean (SD)	Obs.
Adoption (ever)	0.253	1195272	0.256	1089496	0.233	56290	0.209	49486
	(0.435)		(0.436)		(0.423)		(0.407)	
In year 1	0.010	1195272	0.010	1089496	0.015	56290	0.015	49486
	(0.100)		(0.097)		(0.121)		(0.122)	
In year 2	0.076	1195272	0.077	1089496	0.071	56290	0.069	49486
	(0.265)		(0.266)		(0.256)		(0.254)	
In year 3	0.088	1195272	0.089	1089496	0.081	56290	0.068	49486
	(0.283)		(0.285)		(0.273)		(0.251)	
In year 4	0.046	1195272	0.047	1089496	0.037	56290	0.037	49486
	(0.209)		(0.211)		(0.189)		(0.189)	
Reunified	0.538	1195272	0.541	1089496	0.533	56290	0.480	49486
	(0.499)		(0.498)		(0.499)		(0.500)	
Legal Guardian/Relative	0.173	1195272	0.167	1089496	0.208	56290	0.276	49486
	(0.378)		(0.373)		(0.406)		(0.447)	
Censored	0.018	1195272	0.018	1089496	0.005	56290	0.022	49486
	(0.133)		(0.135)		(0.068)		(0.146)	
Reentry from adoption	0.003	281,824	0.003	260,162	0.004	12,539	0.001	9,123
(within 2 years)	(0.053)		(0.053)		(0.067)		(0.033)	
Black	0.254	1195272	0.253	1089496	0.260	56290	0.285	49486
	(0.436)		(0.435)		(0.439)		(0.451)	
White	0.459	1195272	0.444	1089496	0.635	56290	0.607	49486
	(0.498)		(0.497)		(0.481)		(0.488)	
Other	0.077	1195272	0.079	1089496	0.047	56290	0.058	49486
	(0.266)		(0.270)		(0.213)		(0.233)	
Hispanic Ethnicity	0.209	1195272	0.224	1089496	0.057	56290	0.050	49486
	(0.407)		(0.417)		(0.232)		(0.218)	
Female	0.487	1195272	0.487	1089496	0.483	56290	0.479	49486
	(0.500)		(0.500)		(0.500)		(0.500)	
Age at removal	5.018	1195272	5.013	1089496	5.287	56290	4.826	49486
	(4.343)		(4.334)		(4.481)		(4.372)	
Length of FC episode, in days	600.597	1195272	608.247	1089496	545.269	56290	495.099	49486
	(589.884)		(594.724)		(564.259)		(489.383)	
Reason for removal includes Abuse	0.229	1150890	0.233	1045606	0.205	56082	0.170	49202
	(0.420)		(0.423)		(0.404)		(0.376)	

Notes: The data source is AFCARS Foster Care files, 2001-2014. All of the above samples are restricted to first episode foster-care children aged 14 and below, who entered the system by September 2010

data for expenditures on Temporary Assistance for Needy Families (TANF) from the Office of Family Assistance, Administration for Children and Families. Last, state-level data on mandated LGAL legislation effective dates were collected from

the Westlaw database and reports compiled by the Child Advocacy Institute and First Star. [20]

Method and Identification

In order to distinguish the effect of LGALs on the probability of adoption, from potential confounding factors, I exploit state-year variation in mandated LGAL legislation. Specifically, I employ a difference-in-differences empirical strategy to explore whether the probability of adoption changes systematically with LGAL mandates. Formally, the specific relationship between the linear predictors and the probabilities of each of four exit outcomes is given by the multinomial logitistic (MNL) function:

$$p_{ij} = Pr[y_i = j] = \frac{\exp(X'\beta_j)}{\sum_{k=1}^{4} \exp(X'\beta_k)}, \qquad j = 1, ..., 4 \tag{2.1}$$

where

$$X'\beta = \beta_0 + \beta_1 1(LGAL_{ismy}) + \delta X_{ismy} + \alpha_s + \lambda_m + \gamma_y + t_{smy} \tag{2.2}$$

[20]The First Star report can be found at `http://www.firststar.org/wp-content/uploads/2015/02/First-Star-Third-Edition-A-Childs-Right-To-Counsel.pdf`

The dependent variable takes on a value of one if a child exits to an adoptive home, a value of two if a child is reunified, a value of three if a child exits to a legal guardian, and a value of four if a child has not attained permanency—namely, if a child ages out of the system, or if a child remains in foster care for more than four years. I choose the less restrictive multinomial logistic model over the ordinal logistic model as there is no inherent ordering between the four discharge outcomes. Furthermore, I use the MNL as an alternative to duration models given the presence of competing risk. In an MNL model the probabilities of discharge types must sum to one, and thus an increase in one exit probability must be offset by a decrease in the probability of one or more alternatives allowing for direct competition among outcomes. Therefore, when we later expand our analysis to allow adoption in each year of foster tenure to serve as its own category, the MNL will allow us to determine whether an increase in the probability of adoption in a given year is primarily driven by a change in the timing of adoption and/or a decrease in the probability of an alternative discharge reason.

Each outcome category gets its own set of coefficients, but the same set of predictors. The covariates of interest, indicated in Equation (2), include $1(LGAL_{ismy})$, an indicator variable equal to one if child i enters foster care in state s in month m of year y and a mandated LGAL legislation is in effect.[21] State, year, and month fixed effects—α_s, λ_m, and γ_y respectively—are added to

[21] Children who enter the system prior to the legislation are entitled to legal representation once the law goes into effect, we will explore the impact of partial treatment for those children in a later section.

control for time-invariant heterogeneity across states, and time-varying and seasonal shocks to adoption outcomes that are constant across all states. We control for observable individual- and state-level heterogeneity with X_{ismy}, including controls for age at foster care entry, race, ethnicity, gender, population, racial composition, unemployment rate, median household income, and average annual TANF expenditures. Importantly, our parameter of interest, $\hat{\beta}_1$, will be biased if the likelihood of adoption is differentially decreasing/increasing over time in states that enact LGAL legislation. The inclusion of state-specific linear time trends, $\alpha_s \times trend$, thus allows us to interpret the parameter of interest, $\hat{\beta}_1$, as the average deviation from state-specific trends coincident with treatment in treatment states. Last, we estimate the error term allowing for state-level clustering.

The main identifying assumption of our difference-in-differences approach is that adoption outcomes for children in treatment states would have changed in a way similar to children in control states in the absence of the legislative change. In later analysis, we estimate our primary specification including leading indicators, and find no evidence of a violation of this identifying assumption.

Results

Adoption

To examine how the appointment of an LGAL impacts the predicted probability of adoption, we consider the average effect of passing a mandated

LGAL statute on the probability of discharge to an adoptive home versus alternative discharge outcomes—namely reunification, guardianship, aging out of the foster care system, or continued foster care stay. In Table 3, each panel corresponds to a different specification and each column corresponds to a different outcome category; specifically, in columns (1) through (3), we report the estimated effect of treatment on the probability of adoption, reunification, and guardianship, respectively. In Column (4), labeled "Other", we report the estimated effect of treatment on the probability of either aging out of the foster care system or the probability of still residing in the foster care system at the close of our sample period.[22]

The results we depict in Panel A of Table 3 are the estimated effects of LGAL mandates on the probability of each of the four possible outcomes occurring controlling for month-, year-, and state-fixed effects. In Panel B, we add state-specific linear trends, and in Panel C, we add individual-level controls and time-varying state level controls. In Panel A, results indicate that treatment significantly decreases the probability of reunification, and increases the probability or guardianship or other. However, it is with the inclusion of state-specific time trends—panels B and C—that the data reveal systematic empirical regularities in the effect of LGALs on discharge outcomes. In specific, our estimates in panels B and C indicate that LGALs have no statistically significant effect on the probability of adoption, or any alternative discharge type. The sizable difference between the

[22]We report the marginal effects from the MNL model represented by Equation (1).

TABLE 3.
Lawyer Guardian Ad Litem (LGAL) Mandate on Discharge Type

	Adoption (1)	Reunification (2)	Guardianship (3)	Other (4)
Panel A: Fixed Effects				
L-GAL Mandate	-0.016	-0.028***	0.023**	0.021***
	(0.013)	(0.009)	(0.009)	(0.007)
Observations	1,195,272	1,195,272	1,195,272	1,195,272
Pre-Treatment Mean	0.204	0.528	0.203	0.065
Impact (%) at Pre-Treatment Mean	-7.7	-5.3	11.2	32.1
Effect Size	0.038	0.056	0.061	0.076
Panel B: Fixed Effects+State Specific Linear Trends				
L-GAL Mandate	0.003	-0.005	-0.001	0.003
	(0.013)	(0.016)	(0.006)	(0.007)
Observations	1,195,272	1,195,272	1,195,272	1,195,272
Pre-Treatment Mean	0.204	0.528	0.203	0.065
Impact (%) at Pre-Treatment Mean	1.3	-0.9	-0.3	4.1
Effect Size	0.006	0.010	0.002	0.010
Panel C: Fixed Effects+State Specific Linear Trends+ Individual and State-Level Controls				
L-GAL Mandate	0.007	-0.010	0.001	0.003
	(0.013)	(0.015)	(0.007)	(0.007)
Observations	1,195,272	1,195,272	1,195,272	1,195,272
Pre-Treatment Mean	0.204	0.528	0.203	0.065
Impact (%) at Pre-Treatment Mean	3.2	-1.9	0.3	4.4
Effect Size	0.016	0.020	0.002	0.010

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each row represents marginal effects from a multinomial logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

estimates presented in Panel A and those presented in panels B and C suggest that accounting for state trends is important in assessing the impact of an LGAL mandate.

Given that we see no evidence of an increase in the overall propensity to be adopted, we next explore how LGALs affect the time to adoption. In order to conduct this analysis, we focus on four year adoptions, and reestimate Equation

(1) now allowing for seven outcome categories. Specifically, the dependent variable takes on a value of 1 through 4 if a child respectively exits to an adoptive home in their first through fourth year in foster care, a value of 5 if reunified, a value of 6 if discharges to a legal guardian, and a value of 7 otherwise—this includes discharge in more than 4 years, aging out of the system, and continued stay in the system. We present these results in Panel A of Table 4 where each column corresponds to each of the seven outcomes of interest. Estimates reported in columns (1) and (2) depict that LGAL representation increases the likelihood of being adopted in the first year by 0.2 percentage points, and increases the likelihood of adoption in the second year by 0.8 percentage point; both estimates are statistically significant at the 5% level.

To meaningfully interpret the marginal effects reported in Panel A of Table 4, we report the corresponding impact percent of the marginal effect estimates (at the pre-treatment mean) in Figure 5, Panel (a). Additionally, in Panel (b) of Figure 5, we translate the estimated treatment effects into the average annual change in the number of children in each category in an average treatment state.[23] As reported in Panel (b), the effect of an LGAL translates to about seven more adoptions in the first year, 24 more adoptions in the second year, and 28 less adoptions in the third year, for a given state-year. In addition, the results summarized in

[23]The change in the number of children associated with each estimated treatment effect—treatment effects are summarized in Panel C of Table 4—is calculated by multiplying the estimated effect by the average number of children entering the system in a treatment state-year, in the periods prior to treatment.

TABLE 4.
Lawyer Guardian Ad Litem (LGAL) Mandate on Adoption/TPR Within 4 Years

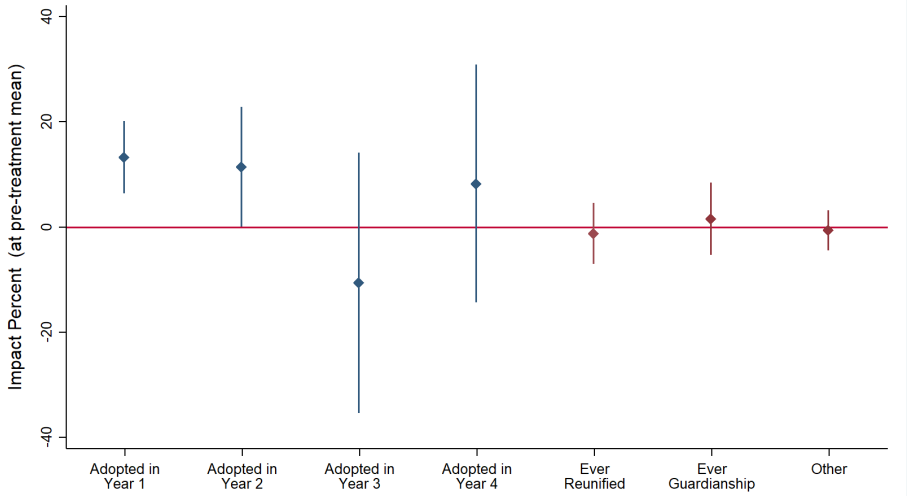
	In Year 1 (1)	In Year 2 (2)	In Year 3 (3)	In Year 4 (4)	Reunification (5)	Guardianship (6)	Other (7)
Panel A: Total Sample							
L-GAL Mandate	0.002***	0.008**	-0.009	0.003	-0.007	0.003	-0.001
	(0.001)	(0.004)	(0.010)	(0.004)	(0.015)	(0.007)	(0.004)
Pre-Treatment Mean	0.015	0.071	0.081	0.037	0.533	0.208	0.213
Impact (%) at Pre-Treatment Mean	13.3	11.5	-10.6	8.3	-1.2	1.6	-0.6
Effect Size	0.020	0.031	0.030	0.015	0.013	0.009	0.003
Observations	1,195,272	1,195,272	1,195,272	1,195,272	1,195,272	1,195,272	1,195,272
Panel B: Conditional on Termination Parental Rights							
L-GAL Mandate	0.007*	0.023*	-0.043***	0.008		0.004	0.002
	(0.004)	(0.012)	(0.009)	(0.011)		(0.003)	(0.011)
Observations	324,088	324,088	324,088	324,088		324,088	324,088
Pre-Treatment Mean	0.061	0.287	0.329	0.151		0.008	0.164
Impact (%) at Pre-Treatment Mean	11.1	7.9	-13.2	5.0		53.1	1.3
Effect Size	0.036	0.050	0.093	0.020		0.036	0.006
Panel C: Termination of Parental Rights							
LGAL Mandate	0.005	0.002	-0.002	0.004	-0.009	0.000	-0.001
	(0.005)	(0.012)	(0.003)	(0.003)	(0.014)	(0.007)	(0.003)
Observations	1,195,122	1,195,122	1,195,122	1,195,122	1,195,122	1,195,122	1,195,122
Pre-Treatment Mean	0.043	0.111	0.065	0.017	0.528	0.201	0.034
Impact (%) at Pre-Treatment Mean	12.1	2.2	-3.2	22.3	-1.7	0.1	-1.6
Effect Size	0.021	0.007	0.010	0.032	0.018	0.001	0.003
State and Month-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual- and State-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each row represents marginal effects from a multinomial logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

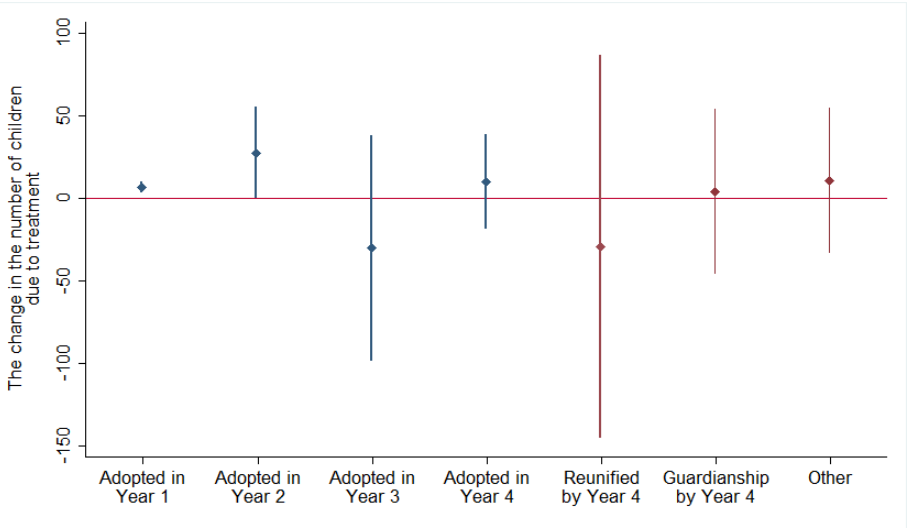
Panel (a) indicate that impacts are largest for the treatment effects corresponding to categories 1 through 4 versus 5 through 7. Thus, legal counsel increases the probability of adoption within the first two years in foster care, and—given that our previous results indicated no significant increase in the probability of ever being adopted—this result is likely primarily driven by decreases in the third year adoptions.

FIGURE 5.
Lawyer Guardian Ad Litem (LGAL) Mandate on Adoption Within 4 Years

(a) LGAL on adoption within 4 years, impact pre-treatment mean



(b) LGAL on adoption within 4 years, the change in the number of children

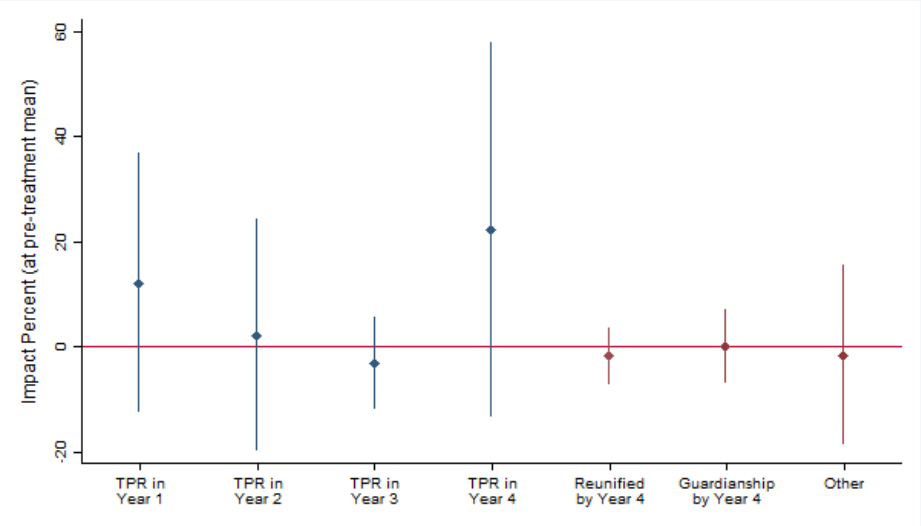


Notes: The figures display estimate transformations of marginal effects, and their 95% confidence intervals, from a multinomial logit regression, accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. Full results from this are shown in Panel A of Table 4. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level.

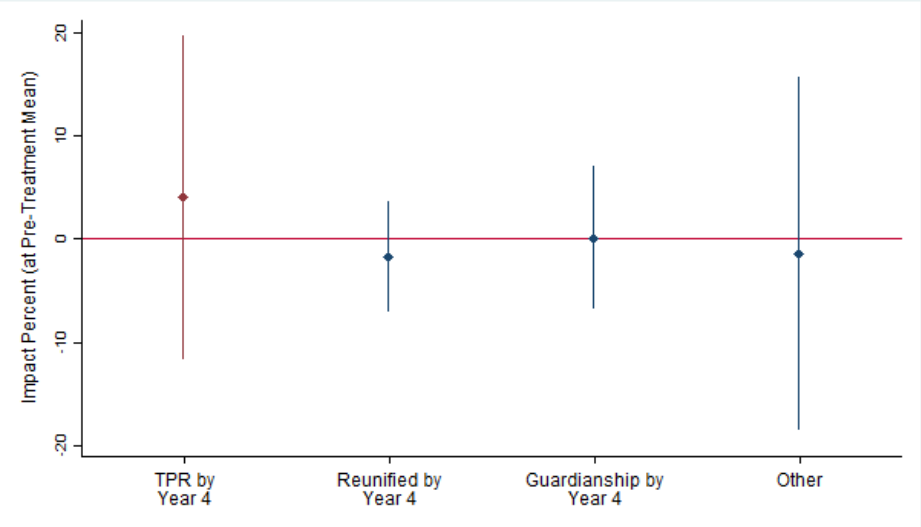
In order to further explore what is driving the increase in adoption within the first two years, we estimate the effect of LGALs on the probability of the termination of parental rights in the first four years. Recall that in order for a foster child to be eligible for adoption parental rights must be terminated, and reunification is not possible; thus, if the increase in two year adoptions is driven in large part by a decrease in reunifications, we must necessarily observe a simultaneous increase in the probability of TPR (within the first two years of foster care tenure). Specifically, we reestimate the seven category version of Equation (1) now defining category one through four as TPR in years one through four of foster care tenure, respectively. Category 5 is defined as reunification within four years; category six is defined as discharge to a legal guardian (in the absence of TPR) within four years; and category seven—referred to as "Other"—includes aging out within four years, TPR in more than four years, or reunification/guardianship in more than four years. The results of this analysis are summarized in Panel (a) of Figure 6 and Panel C of Table 4. Our results indicate that the effect of LGALs on the probability of TPR in each of the first four years of foster care tenure is statistically indistinguishable from zero. Furthermore, our results indicate that impact of LGALs on the probability of reunification is close to zero. This evidence suggests that the positive effect of legal counsel on the probability of adoption within two years is not likely driven by substitution away from reunification but instead is driven by substitution away from third year adoptions.

FIGURE 6.
Lawyer Guardian Ad Litem (LGAL) Mandate on Termination of Parental Rights (TPR)

(a) LGAL on TPR in each year, impact pre-treatment mean



(b) LGAL on TPR within 4 years, impact pre-treatment mean



Notes: The figures display estimate transformations of marginal effects, and their 95% confidence intervals, from a multinomial logit regression, accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. Full results from this are shown in Panel C of Table 4. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level.

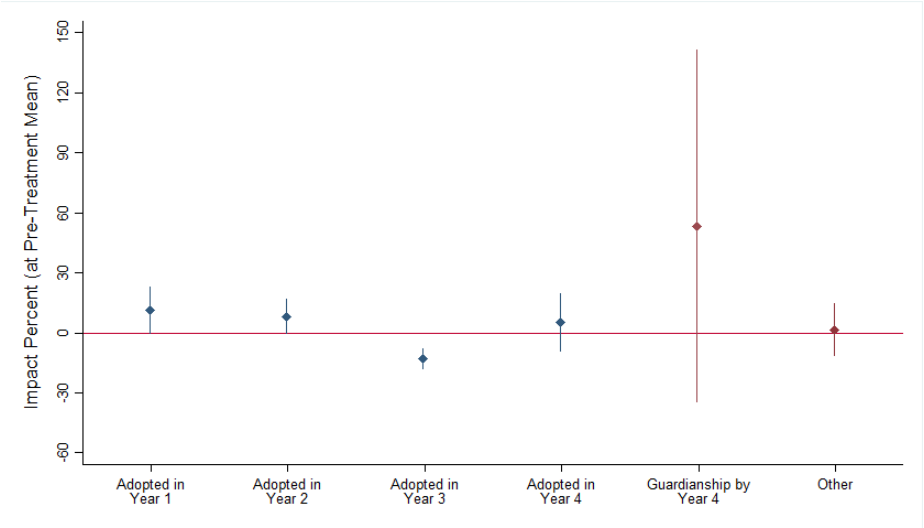
Last, we conduct the analysis presented in Panel A of Table 4, but now restricting the sample to foster children for whom parental rights have been terminated we summarize the results from this analysis in Figure 7 and Panel B of Table 4. The results summarized in panels (a) and (b) of Figure 7 mirror those summarized in panels (a) and (b) of Figure 5. Despite the loss of power, we still observe a significant positive treatment effect on the likelihood of adoption in the first two years of foster care tenure, and we now observe a significant decrease in the probability of adoption in the third year of foster care tenure. Specifically, in the average state, treatment increases the average annual number of adoptions in the first and second year of foster care tenure by about 30 and 90 respectively; furthermore, we see that treatment simultaneously leads to about 148 less adoptions in the third year of foster care tenure. These results lend further evidence that the increase in the probability of adoption in the first two years is most likely driven by a decrease in average length of time to adoption, and not a decrease in alternative discharge types—reunification, guardianship, or aging out.

PARTIAL TREATMENT AND SPECIFICATION CHECK

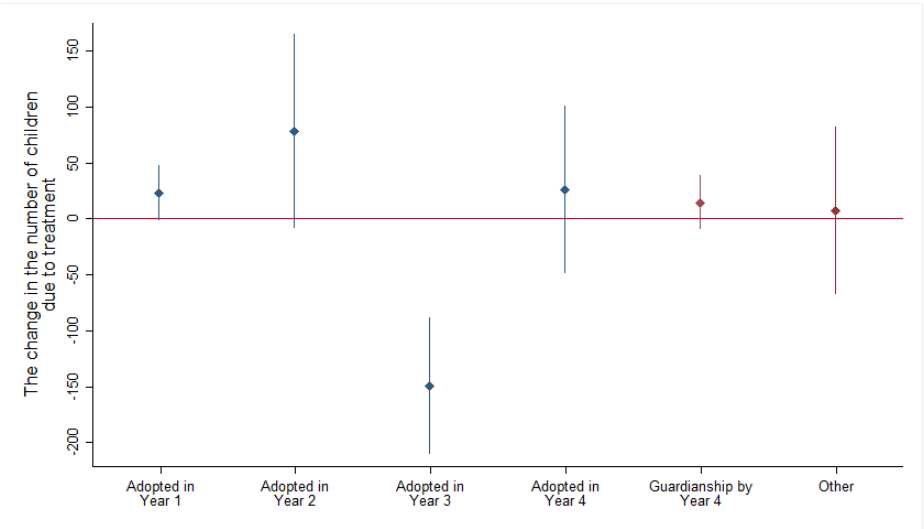
As previously mentioned, children who enter the foster care system prior to the LGAL mandate will receive an attorney after the passage of the law; the results of our partial treatment analysis on adoption in the first and second year of foster care tenure are summarized in Figure 8. Note that for this analysis we estimate the seven category multinomial logit model, but we summarize only the

FIGURE 7.
Lawyer Guardian Ad Litem (LGAL) Mandate on Adoption Within 4 years, Conditional on Termination of Parental Rights

(a) Impact percent pre-treatment mean



(b) The change in the number of children



Notes: The figures display estimate transformations of marginal effects, and their 95% confidence intervals, from a multinomial logit regression, accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. Full results from this are shown in Panel B of Table 4. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level.

estimates corresponding to adoption in the first and second year (category one and two) in panels (a) and (b) respectively. In both panels, we present estimates from 16 separate models, each estimate corresponds to the differential effect of LGAL representation for children who enter the system some time prior to the passage of the LGAL mandate. Specifically, we estimate the following

$$p_{ij} = Pr[y_i = j] = \frac{\exp(X'\beta_j)}{\sum_{k=1}^{4} \exp(X'\beta_k)}, \qquad j = 1, ..., 7 \tag{2.3}$$

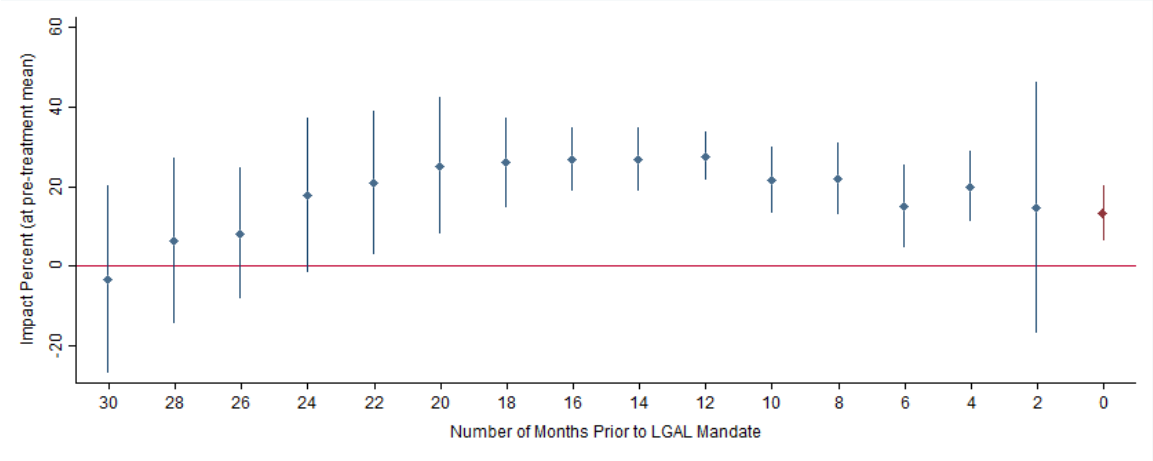
where

$$X'\beta = \beta_0 + \beta_1 1(LGAL_{ismy}) + \beta_2 1(EntryPrior_{ismy}) + \delta X_{ismy} + \alpha_s + \lambda_m + \gamma_y + t_{smy} \tag{2.4}$$

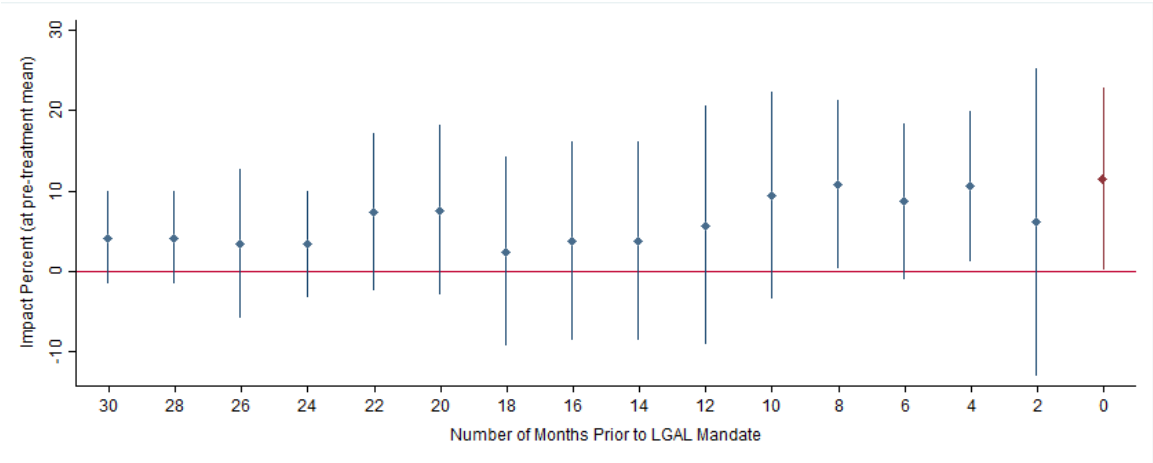
Here, $EntryPrior_{ismy}$ is equal to 1 if a child enters the system within x months prior to the law, and zero otherwise. This variable allows us to estimate the effect of the LGAL mandate for children entering prior to the effective date, controlling for the effect of our baseline treatment (ie, $LGAL_{ismy}$); this partial treatment effect is represented by β_2, and it is this parameter estimate (corresponding to categories one and two) that we summarize in panels (a) and (b) of Figure 8.

FIGURE 8.
Lawyer Guardian Ad Litem (LGAL) on Adoption, Within 2 Years,
Alternative Definitions of Treatment

(a) Adoption in the first year, impact



(b) Adoption in the second year, impact



Notes: The figures display estimate transformations of marginal effects, and their 95% confidence intervals, from multinomial logit regressions, accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. Each estimate corresponds to a separate regression. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level.

We start by re-estimating our baseline specification (the baseline results are indicated in red). Then, we estimate the above model, in which we control for the baseline effect of treatment, and allow $EntryPrior_{ismy}$ to capture children entering the system within the 30 months prior to the effective date of the LGAL statute—these are the estimates reported on the furtherest to left of panels (a) and (b). Assigning alternative treatment successively earlier, in two-month intervals, we reveal that children who enter the system prior to the treatment year appear to experience an increase in adoption within their first year as a result of later treatment (Panel (a)). Specifically, we observe that the magnitude of the treatment effect increases over the first 12 months prior to the mandate passage—before the treatment effect begins to decay. This suggests that for children who enter within 12 months before a mandate is in effect, legal representation significantly impacts the probability of adoption in the first year (even more so than those children who enter right around the passage of the law). The estimates presented in Panel (b) suggest that lawyers do not similarly significantly increase the probability of adoption in the second year for children who receive a lawyer within two years of the LGAL mandate.[24]

To address concerns that our baseline results, Figure 5, are driven by differential trends in adoption across children in treatment and control states, we conduct an event study by re-estimating the preferred MNL specification allowing

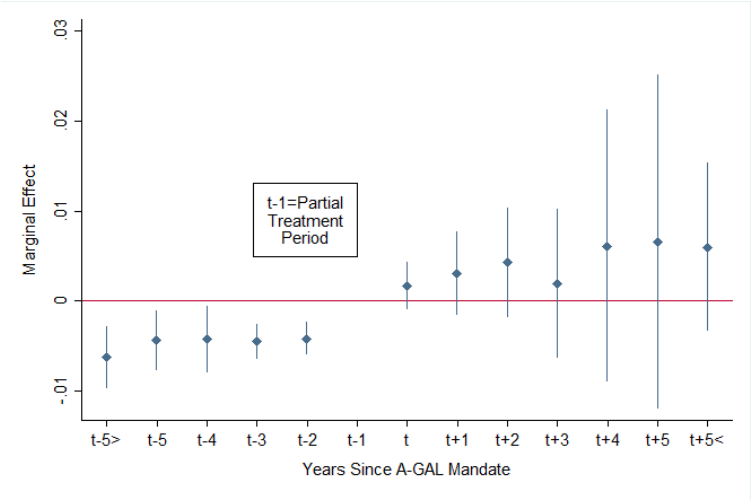
[24]Although the analysis is not presented here, we find no significant partial treatment effects on the probability of achieving any of the remaining five discharge categories.

for different levels across all pre- and post-treatment years. Specifically, we add separate indicator variables for one to five years prior to treatment, six or more years prior to treatment, the year of treatment, one to five years post-treatment, and six or more years post treatment; these results are summarized in Figure 12 and Table A1. In Panel (a), we summarize results corresponding to the outcome of adoption within one year of entry; the reference category is "one year prior to treatment". Although leading indicators do not suggest a general trend prior to treatment, all leading indicators are statistically different from zero and our lagged indicators are statistically insignificant. However, given that our partial analysis reveals that children who enter within the first year of treatment (the reference category) experience significant increases in the probability of first year adoptions these results are as expected, and do not suggest a violation of our identifying assumption.

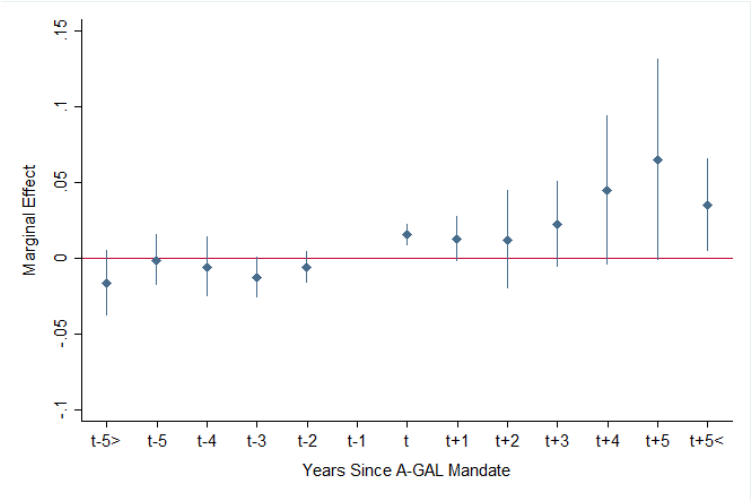
The results in Figure 12, Panel (b), correspond to the outcome of adoption in year two of foster care tenure, these estimates similarly do not suggest a violation of our identifying assumption as the marginal effects on the lead indicators are close to zero. Importantly, note that in this case, unlike before, we do not expect our lead indicators to be significantly negative, because we do not observe any evidence of a significant effect of an LGAL mandate on the probability of adoption in year two of foster care tenure for children who are partially treated—Figure 8,

FIGURE 9.
Lawyer Guardian Ad Litem (LGAL) on Adoption, Within 2 Years,
Before and After Effective Legislation years

(a) Adoption in the first year



(b) Adoption in the second year



Notes: The figure displays the marginal effects and their 95% confidence intervals for the leading indicators and lagged treatment effects from a multinomial logit regression, accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. Full results from this are shown in Table A1 The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level.

Panel (b). We also observe a significant effect in the treatment year; the positive effect of treatment appears to persist beyond the first year.

Subgroup Analysis

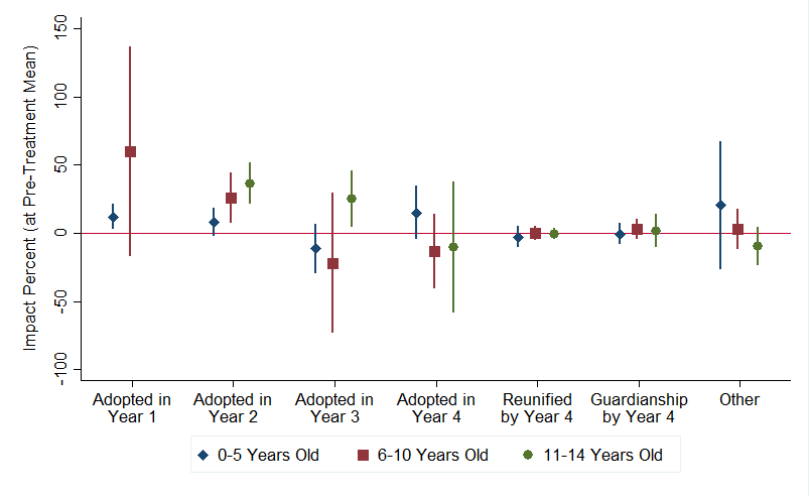
As previously discussed, individual child characteristics such as age, race, and gender, are indicators of a child's probability of being adopted, and tenure in the foster care system. Therefore, we explore how mandated legal counsel for foster children during permanency planning differentially impacts subgroups of the foster care population. First, we explore the heterogeneous effect of an LGAL mandate on the probability of adoption within the first four years across age groups—children ages 0-5 years old, ages 6-10 years old, and ages 11-14 years old—we depict the results of this analysis in Figure 10 and Table 5. The results depicted in Panel (a) of Figure 10 reveal that LGALs have the smallest impacts on those aged 0-5 years old, and the largest impacts for those aged 11-14. Specifically, LGALs lead to a significant 37% and 26% increase in adoption for 11-14 years olds (at the pre-treatment mean) in the second and third year of foster care tenure respectively—on average it takes a child above the age of eleven 3 years to discharge to an adoptive home. Additionally, note that results summarized in Panel (a) of Figure A1 indicate that for children ages 11 to 14 years old attorney representation increases the overall probability of being adopted within fours years of foster care tenure. For adolescents, legal counsel leads to a 21% increase in the four year adoptions (at the pre-treatment mean); this appears to be primarily driven by a decrease in the

probability of aging out or continued foster care stay. These results are all the more striking given that adolescents were more likely than younger children to receive legal counsel prior to mandated LGAL, but still experience the largest impact on average. These results not only highlight the importance of legal counsel for disproportionately disadvantaged foster youth, but also provide suggestive evidence that our estimates likely capture a lower bound estimate of the local effect of child lawyers on adoption outcomes for individual children.

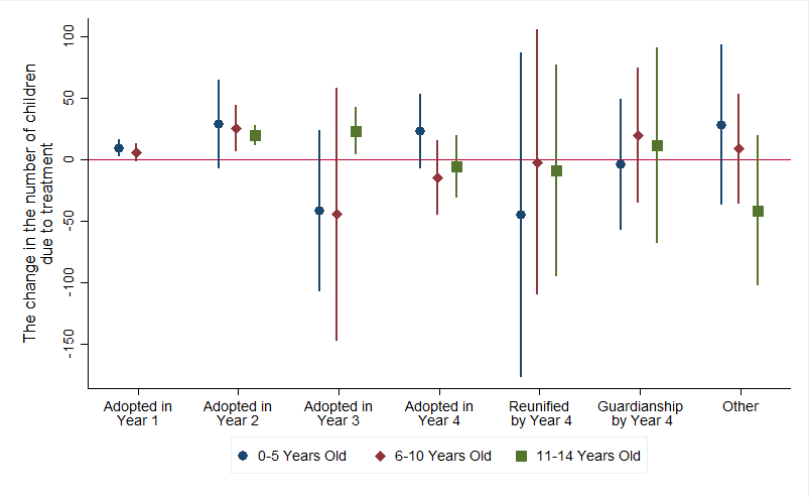
Next, we explore the impact of LGAL mandates on the probability of adoption in each of the first four years across race, gender, and reason for removal—these results are presented in Figure 11 and Table 6. In panels (a) and (b) of Figure 11, we present the impacts at the pre-treatment mean and corresponding average annual number of children per state, respectively, for the analysis conducted across black and white foster child populations. LGALs significantly increase the probability of adoption in the second year for both black and white foster children; the corresponding impacts (at the pre-treatment mean) of LGALs are slightly larger for white foster children. In panels (c) and (d) of Figure 11, we present similar results across gender, indicating that the effect of LGALs on adoption in the first 2 years is not driven by a single gender; furthermore, the impacts of LGALs on discharge outcomes appear similar across males and females—Panel (b).

FIGURE 10.
Lawyer Guardian Ad Litem (LGAL) on Adoption Within 4 Years,
by Age Group

(a) Impact percent (at pre-treatment mean)



(b) Change in the number of children



Notes: The figure displays the marginal effects and their 95% confidence intervals from multinomial logit regressions, accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. Full results from these regressions are shown in Table 5. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level.

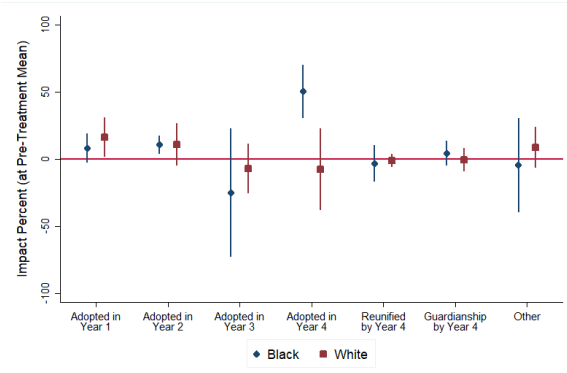
TABLE 5.
Lawyer Guardian Ad Litem (LGAL) Mandate on Adoption Within 4 Years, by Age Group

	In Year 1 (1)	In Year 2 (2)	In Year 3 (3)	In Year 4 (4)	Reunification (5)	Guardianship (6)	Other (7)
Panel A: Ages 0-5							
L-GAL Mandate	0.003***	0.009	-0.012	0.007	-0.012	0.000	0.005
	(0.001)	(0.005)	(0.010)	(0.004)	(0.019)	(0.007)	(0.008)
Observations	745234	745234	745234	745234	745234	745234	745234
Pre-Treatment Mean	0.023	0.102	0.105	0.045	0.483	0.206	0.114
Impact (%) at Pre-Treatment Mean	0.121	0.083	-0.113	0.151	-0.024	0.000	0.048
Effect Size	0.023	0.027	0.037	0.030	0.024	0.000	0.015
Panel B: Ages 6-10							
L-GAL Mandate	0.002	0.007***	-0.013	-0.004	0.001	0.007	-0.000
	(0.001)	(0.003)	(0.015)	(0.004)	(0.015)	(0.007)	(0.005)
Observations	283144	283144	283144	283144	283144	283144	283144
Pre-Treatment Mean	0.003	0.029	0.057	0.032	0.591	0.217	0.371
Impact (%) at Pre-Treatment Mean	0.598	0.258	-0.218	-0.131	0.001	0.033	-0.000
Effect Size	0.035	0.043	0.055	0.022	0.001	0.018	0.000
Panel C: Ages 11-14							
L-GAL Mandate	NA	0.006***	0.007**	-0.002	0.004	0.011	-0.026
	NA	(0.001)	(0.003)	(0.004)	(0.008)	(0.016)	(0.018)
Observations	166642	166642	166642	166642	166642	166642	166642
Pre-Treatment Mean		0.016	0.027	0.015	0.635	0.201	0.633
Impact (%) at Pre-Treatment Mean		0.368	0.256	-0.098	0.006	0.055	-0.041
Effect Size		0.048	0.046	0.012	0.008	0.029	0.057
State and Month-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual- and State-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

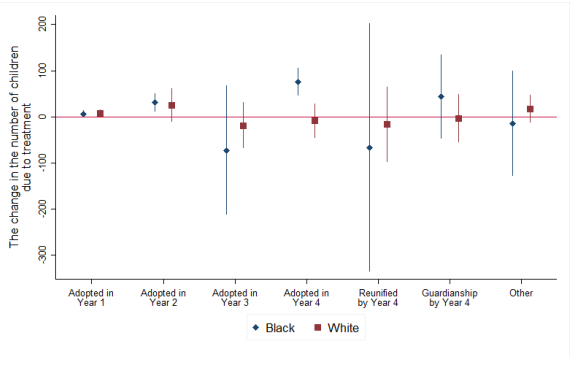
Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each row represents marginal effects from a multinomial logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

FIGURE 11.
Lawyer Guardian Ad Litem (LGAL) on Adoption Within 4 Years, by Subgroups

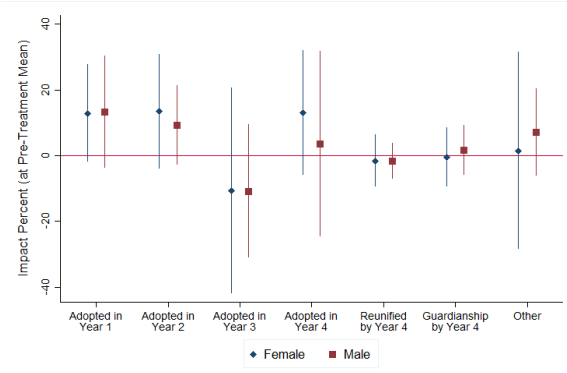
(a) By race, impact percent (at pre-treatment mean)



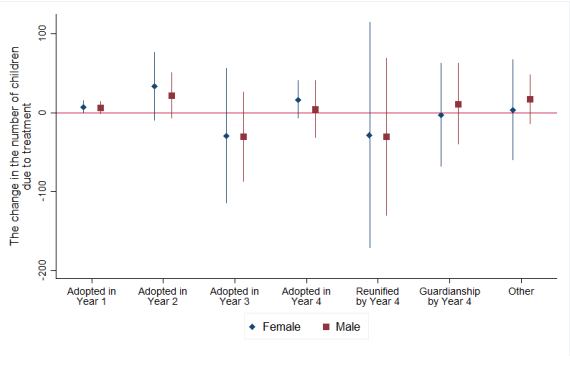
(b) By race, change in the number of children



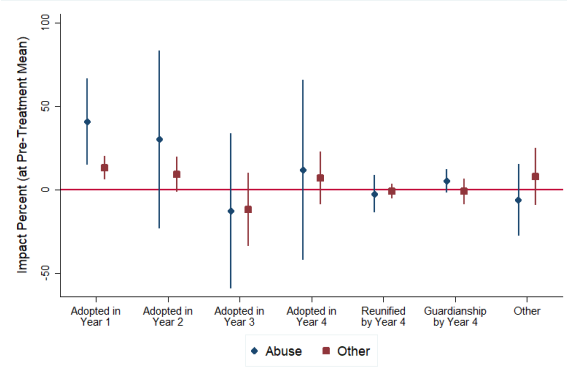
(c) By gender, impact percent (at pre-treatment mean)



(d) By gender,change in the number of children



(e) By reason for removal,impact percent (at pre-treatment mean)



(f) By reason for removal,change in the number of children

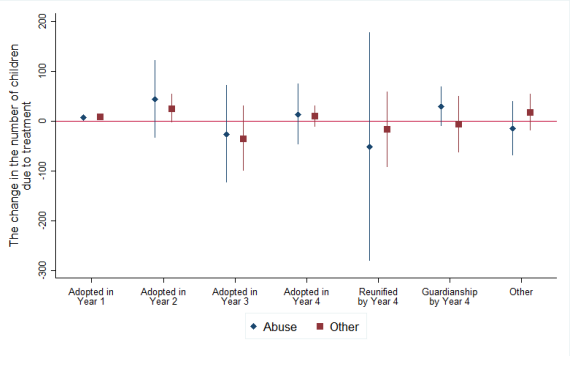


TABLE 6.
Lawyer Guardian Ad Litem (LGAL) Mandate on Adoption Within 4 Years, by Subgroup

	In Year 1 (1)	In Year 2 (2)	In Year 3 (3)	In Year 4 (4)	Reunification (5)	Guardianship (6)	Other (7)
Panel A: Black							
L-GAL Mandate	0.001	0.008***	-0.017	0.018***	-0.014	0.014	-0.011
	(0.001)	(0.002)	(0.017)	(0.004)	(0.032)	(0.013)	(0.011)
Observations	304175	304175	304175	304175	304175	304175	304175
Pre-Treatment Mean	0.016	0.070	0.071	0.036	0.491	0.249	0.256
Impact (%) at Pre-Treatment Mean	0.082	0.109	-0.245	0.508	-0.028	-0.058	0.042
Effect Size	0.015	0.033	0.065	0.085	0.027	0.035	0.023
Panel B: White							
L-GAL Mandate	0.003***	0.010*	-0.005	-0.003	-0.006	-0.001	0.003
	(0.001)	(0.005)	(0.006)	(0.005)	(0.008)	(0.007)	(0.004)
Observations	549204	549204	549204	549204	549204	549204	549204
Pre-Treatment Mean	0.014	0.069	0.083	0.037	0.548	0.199	0.197
Impact (%) at Pre-Treatment Mean	0.192	0.145	-0.058	-0.090	-0.011	-0.007	0.014
Effect Size	0.026	0.036	0.017	0.017	0.012	0.003	0.007
Panel C: Female							
L-GAL Mandate	0.002*	0.010	-0.008	0.005	-0.006	0.001	-0.004
	(0.001)	(0.006)	(0.012)	(0.004)	(0.019)	(0.008)	(0.006)
Observations	581872	581872	581872	581872	581872	581872	581872
Pre-Treatment Mean	0.016	0.072	0.080	0.037	0.528	0.215	0.204
Impact (%) at Pre-Treatment Mean	0.129	0.136	-0.104	0.133	-0.011	0.006	-0.019
Effect Size	0.020	0.037	0.029	0.024	0.012	0.003	0.009
Panel D: Male							
L-GAL Mandate	0.002	0.006	-0.009	0.001	-0.007	0.005	0.001
	(0.001)	(0.004)	(0.008)	(0.005)	(0.013)	(0.008)	(0.003)
Observations	613400	613400	613400	613400	613400	613400	613400
Pre-Treatment Mean	0.014	0.069	0.082	0.037	0.538	0.201	0.222
Impact (%) at Pre-Treatment Mean	0.133	-0.094	0.106	-0.038	0.013	0.026	0.004
Effect Size	0.019	0.025	0.031	0.007	0.015	0.014	0.002
Panel E: Abuse							
L-GAL Mandate	0.002***	0.013	-0.008	0.004	-0.015	0.011**	-0.007
	(0.001)	(0.012)	(0.015)	(0.009)	(0.034)	(0.005)	(0.005)
Observations	263474	263474	263474	263474	263474	263474	263474
Pre-Treatment Mean	0.005	0.044	0.063	0.034	0.618	0.174	0.297
Impact (%) at Pre-Treatment Mean	0.407	0.301	-0.126	0.119	-0.025	-0.065	0.024
Effect Size	0.028	0.056	0.031	0.021	0.032	0.030	0.016
Panel F: Other reason for removal							
L-GAL Mandate	0.002***	0.007*	-0.010	0.003	-0.003	0.000	0.000
	(0.001)	(0.004)	(0.010)	(0.003)	(0.009)	(0.008)	(0.004)
Observations	887416	887416	887416	887416	887416	887416	887416
Mean Pre-Treatment Mean	0.017	0.078	0.086	0.038	0.511	0.216	0.196
Impact (%) at Pre-Treatment	0.133	0.094	-0.117	0.072	-0.005	0.001	0.001
Effect Size	0.022	0.026	0.034	0.013	0.005	0.001	0.000
State and Month-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual- and State-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each row represents marginal effects from a multinomial logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

Last, we summarize the results from analysis conducted across reason for removal—specifically, if reason for removal stated abuse versus no stated abuse—these results are presented in panels (a) and (b) of Figure 11. LGALs increase the probability of adoption in the first year of foster care tenure for both populations; however, the impact of this effect is far larger for those who were placed in out-of-home care due to abuse—specifically the impact of legal counsel (at the pre-treatment mean) is 41% for those who were abused. Overall, the results from the subgroup analysis lend modest supporting evidence for the hypothesis that legal counsel in dependency hearings may matter more for certain populations that typically face the greatest preconceptions in court, and are associated with longer foster care tenure and lower probabilities of permanency placement, such as older and abused children.

Reentry

We now turn to examine how mandated LGAL representation impacts placement stability. Specifically, we will look at how attorney representation upon system entry affects the probability of foster care reentry, within two years, conditional on discharge to an adoptive home. Due to late treatment dates, we are unable to look at system reentry within a longer time span, this limits our ability to explore if legal counsel results in delayed system reentry. In addition, our results do not speak to the possibility that LGALs mitigate factors that result in quicker adoption failures (specifically within 2 years), but not factors that associate with

later adoption failures. As such, our results will speak to the impact of lawyers on short-term reentry; to my knowledge, this is the first study to look at the effect of foster child legal counsel on foster care reentry from an adoptive home. The analysis presented here will shed light on how LGAL counsel affects placement stability. Furthermore, these results will allow us to speak to the concern that a lawyer's efforts to expedite the adoption process may lead to tenuous adoption matches (Slowriver and Zinn, 2008).

Generally speaking, system reentry from an adoptive home is a rare outcome, in the sample of children who discharge from the system to an adoptive home by September 2012—about 285,050 children—only 0.8-percent ever reenter foster care.[25] Of the 2,424 children who experience reentry from an adoptive home, about 35% reenter foster care by their second year.

To explore how legal counsel affects foster care reentry from adoption, we estimate a logistic regression where the outcome of interest is equal to one if the adopted child reenters the foster care system in two years, and zero otherwise.[26] The covariates of interest in the model are those described in Equation (2), the results of this analysis are summarized in Column (1) of table 7.[27] The results

[25]In order to identify the impact of legal counsel on reentry within 2 years of discharge, we must additionally restrict the sample to children who exit by September 2012, and children who are below the age of 16 upon exit. These restrictions ensure that children contributing to the sample can possibly reenter within two years. Note, we now relax the restriction that children must have entered the system by September 2010.

[26]We also examine the impact of LGAL representation on re-entry from reunification and guardianship; in both cases the outcome of interest still takes on a value of one if the discharged child reenters the system within two years, and zero otherwise.

[27]In this analysis, we also control for a child's length of stay in the foster care system.

suggest that legal counsel leads to a significant decrease in the probability of foster care reentry within two years of adoption; the magnitude of the impact at the pre-treatment mean (of two year reentry from adoption) is 86%. In addition, we look at the effect of legal counsel on reentry within two years of other permanency placements—columns (2) and (3). Although we see positive treatment effects on two year reentries from both reunification and guardianship, the estimates are not statistically distinguishable from zero; this is unsurprising, as earlier analysis did not suggest that LGALs have a significant impact on the overall probability of reunification or guardianship. This analysis finds that children represented by legal counsel experience more stable adoption placement within at least the first two years of discharge; this evidence alleviates some of the concern that legal representation results in matches that are more susceptible to dissolution.

TABLE 7.
Lawyer Guardian Ad Litem (LGAL) Mandate on Foster Care Reentry Within 2 Years

	From Adoption (1)	From Reunification (2)	From Guardianship (3)
LGAL Mandate	-0.004***	0.005	0.005
	(0.001)	(0.005)	(0.003)
Observations	246,697	692,363	216,317
Pre-Treatment Mean	0.005	0.140	0.064
Impact (%) at Pre-Treatment Mean	-85.5	3.8	8.6
Effect Size	0.073	0.017	0.023
State and Month-by-Year Fixed Effects	Yes	Yes	Yes
State-Specific Linear Time Trends	Yes	Yes	Yes
Individual- and State-Level Controls	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each row represents marginal effects from a logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, length of foster care episode, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

Conclusion

We offer strong evidence that legal representation for children in dependency hearings leads to a decrease in the total time adopted children spend in foster care. Estimates imply that the number of adoptions in the first two years would increase by over 30 per year in the average state; this increase is primarily driven by a decrease in the number of adoptions in the third year, and not a decrease in reunification or other alternatives. Thus, these results also indicate that increases in the probability of adoption at the hands of lawyers are not primarily a result of substitution away from reunification efforts. In addition to documenting the overall average effect of LGAL representation on adoption, we also find that legal representation has larger impacts for certain foster care populations that are most at risk of aging out of the foster care system without permanency. This suggests that legal counsel may play a role in mitigating adoption disparities across race, age, socio-economics status, and other. Last, we find evidence that legal counsel representation decreases the probability of short-term system reentry from an adoptive home.

These results imply that legal representation for foster children may have considerable impacts on financial costs and potential long-term societal costs. Although there is considerable heterogeneity across states and locales, one study estimates the cost of providing legal counsel for children in dependency hearings to be between \$1,500-\$4,500 per child per year (Taylor, 2009). In a 2010 report,

it was estimated that the average annual net savings for one child being adopted out of foster care was $15,480 (Zill, 2011).[28] Thus, although legal counsel doesn't come cheap, the use of child attorneys in dependency hearings can lead to millions in annual government savings by expediting the adoption process, and minimizing subsequent foster care reentry.[29] In addition, accelerated adoptions may improve child well-being and ultimately mitigate potential long-run societal costs associated with foster care tenure. Although this study does not directly speak to the impact of legal counsel on the quality of care received by adopted children, studies suggest that children may benefit from the act of formalizing adoptions alone as it gives children a sense of permanence and belonging (Triseliotis and Hill, 1990; Triseliotis, 2002; Hansen, 2007; Taylor, 2009).

By exploiting temporal variation in state-level statutes mandating a foster child's right to legal counsel, this study is the first to provide causal evidence of how attorneys may better-facilitate permanency outcomes for foster youth. However, states are continuously updating this legislation in an attempt to improve on the quality of representation in dependency hearings. As such, additional consideration may identify best practices. For example, attorney salaries, caseload maximums, and training requirements are all policy innovations that may contribute to child outcomes. In addition, over 50% of state statutes mandate that

[28] In 2010, the national average costs for one child to remain in the foster care system for just one year was $25,782.

[29] The annual net savings for one child to be adopted out of foster care with the representation of an attorney is between $11,000-$14,000.

counsel for children be client directed—that is, the lawyer must only represent the child's expressed interests as opposed to the perceived "best interests" of the child—and many advocate for this specific type of legal representation for foster youth. However, little empirical evidence speaks to the merits of client-directed child attorneys. The empirical regularities we identify here clearly encourage a better understanding of how court reform ultimately can effect the outcomes of vulnerable populations.

CHAPTER III

MARIJUANA USE AND CHILD MALTREAT: EVIDENCE FROM MEDICAL MARIJUANA LAWS

Introduction

The Federal Child Abuse Prevention and Treatment Act (CAPTA) defines child abuse and neglect as "any recent act or failure to act on the part of a parent or caregiver [that] results in death, serious physical or emotional harm, sexual abuse or exploitation," or "an act or failure to act [that] presents an imminent risk of serious harm."[1] Over the last five years, the rate of child victimization has steadily increased, with almost two-million maltreatment reports assessed by Child Protective Services (CPS) in 2016 alone. In roughly one quarter of these reports, the alleged maltreatment was substantiated. Given the unfortunate prospect of continued child maltreatment, it is important to identify and understand individual, familial, and societal factors that contribute to or mitigate the risk of child abuse and neglect.

The Center for Disease Control and Prevention lists caretaker substance abuse as a leading risk factor in the perpetration of child abuse and neglect.[2] In a 2016 report, many states noted increases in child victimization being associated

[1](42 U.S.C.A. §5106g)

[2]Risk factors are characteristics (of an individual, family, or community) associated with child abuse and neglect—they may not be direct causes (CDC, 2018).

with parental drug use, while existing literature also demonstrates that children of substance-abusing parents are more likely to experience abuse or neglect than children in non-substance abusing households (Famularo et al., 1992; Markowitz and Grossman, 2000; Paxson and Waldfogel, 2002; Walsh et al., 2003; Hohman et al., 2004; Cunningham and Finlay, 2013).[3]

Despite this literature—existing efforts largely pertain to the relationship between parental substance-use disorder and child maltreatment—relatively little is known about the effect of caretaker marijuana use on child abuse and neglect. Moreover, as public policy moves toward increasing access to marijuana, and public acceptance for the substance continues to grow, the potential effects of marijuana use on child maltreatment are of substantial policy relevance.

Research has shown that parental perpetration of child physical abuse correlates positively with marijuana access and use. Yet, the inability to model selection into marijuana use limits the evidence to correlational, and not necessarily causal (Freisthler et al., 2015, 2017). Here, we exploit the exogenous temporal variation in state-level medical marijuana laws (MMLs) to explore the effect of marijuana use on rates of child maltreatment. In a difference-in-differences framework, we consider whether states that enact MMLs experience coincident changes in the rates of child maltreatment reporting and/or substantiation, with our focus on the rates of reported child maltreatment, neglect, physical abuse, and the substantiation of any such reports.

[3]Source: Child Maltreatment 2016, U.S. Department of Health and Human Services.

Child maltreatment data do not identify marijuana use at the individual caretaker level. As such, we are unable to identify the first-stage effect of MML on use. However, existing literature has shown that the legalization of medical marijuana increases general population use of marijuana (Mark Anderson et al., 2013; Chu, 2014; Wen et al., 2015; Pacula et al., 2015). For example, adopting a similar identification strategy to ours below, Wen et al. (2015) finds that MML increases reported adult marijuana use by 14 percent at the extensive margin and, among users, induces a 15-percent increase in frequency. Thus, as we explore whether MML similarly determines the perpetration of child maltreatment, we might imagine that we identify a lower-bound estimate on the marginal effect of actual marijuana consumption on child maltreatment. Moreover, policies that further advance marijuana access—the advent of recreational marijuana legalization is the obvious extension—would arguably have larger effects on child maltreatment than the corresponding effects of MMLs.

Using administrative data on U.S. child-maltreatment reports from the years 2003 to 2014, we find that state-level marijuana legalization coincides with decreases in the average rate of physical abuse accusations (by 2.2 per 100,000 adults) and decreases the average rate of substantiated physical abuse perpetration (by 0.4 per 100,000 adults). At the same time, there is no evidence of coincident increases in the rates of neglect reporting/substantiation; robustness checks indicate no evidence that these results are driven by significant differences in maltreatment

trends across states prior to MML passage. Furthermore, the fact that MMLs decreases substantiation rates indicate that marijuana liberalization decreases not only reporting behavior, but also actual perpetration of child physical abuse.

As marijuana use and the associated effects of use may differ across subgroups, we also investigate the potential heterogeneity, which suggests that marijuana access has a larger effect on the perpetration of physical abuse by male caretakers, white caretakers, and those between the ages 26 to 44. These findings are consistent with the heterogeneity analysis presented in previous studies that explore the effect of MMLs on general marijuana use and related outcomes (Mark Anderson et al., 2013; Chu, 2014; Wen et al., 2015).

For those 20 states without a medical marijuana law currently in place, our study indicates that MMLs have positive child welfare externalities through reductions in the perpetration of child abuse. Physical child abuse has been linked to a plethora of negative health and human capital outcomes for the victims, and as such these laws also serve to mitigate some of the potential long term societal costs associated with the perpetration of child abuse (Edwards et al., 2003; Currie and Tekin, 2006; Springer et al., 2007; Lansford et al., 2002; Norman et al., 2012). Furthermore, in so far as MMLs increase general population marijuana use, our estimates indicate what the average state could anticipate in terms of the child maltreatment consequences of recreational marijuana legalization or other marijuana liberalization policies. For example, using parameter estimates from Wen

et al. (2015), we find that 1320 more adults using marijuana explains a drop of about 30 physical abuse reports (per 100,000) in a given state-year.[4].

Background

Medical Marijuana Laws in the United States

Marijuana is thought to have been introduced in the U.S. in the early 1600s, being outlawed by the federal government in 1970 with the passage of the Controlled Substances Act (CSA). In the late 1980s and early 1990s, smokable marijuana was discovered to have positive effects on patients suffering from nausea related to cancer or AIDS, and with evidence of marijuana's medicinal effects and the support of advocacy groups, a wave of state-level medical-marijuana legislation began in 1996 (Pacula et al., 2002). As of May 2014, 20 states and the District of Columbia had a medical-marijuana law in effect—states and corresponding MML effective dates are presented in Table 8.[5]

Medical marijuana laws allow patients with designated diseases and syndromes to purchase and use marijuana as a treatment. Although specific components of an MML may differ across states, under typical MMLs, eligible patients will first obtain a recommendation for medical marijuana treatment from

[4]We find that in a given state-year MMLs decrease the rate of physical abuse by roughly 30 reports per 100,000 adults. The results in (Wen et al., 2015) translate to an increase in the rate of reported adult marijuana use (on the extensive margin) by 1320 per 100,000 adults in a given state-year

[5]See `https://medicalmarijuana.procon.org/view.resource.php?resourceID=000881` for additional detail regarding laws and legal documents.

a qualified doctor. With the doctor's recommendation for medicinal marijuana treatment—typical conditions include AIDS, anorexia, arthritis, cancer, chronic pain, glaucoma, migraines, severe nausea and seizures—a patient can then be issued a medical-marijuana patient-identification card by the state. A patient with a valid ID, and the patient's caregiver, are legally permitted to possess an amount of marijuana, through purchase from a nonprofit retail dispensary licensed by the state and/or cultivation at home on a not-for-profit basis.

Given the constraints on marijuana procurement for medical treatment, MMLs should in principle only increase marijuana use among legally qualified patients. However, research has shown that the passage of MMLs likely increases marijuana use among nonpatients (Hathaway et al., 2011; Cerdá et al., 2012; Mark Anderson et al., 2013; Chu, 2014; Pacula et al., 2015; Wen et al., 2015). There are several channels through which MMLs may increase marijuana use among nonpatients. The passage of an MML represents a pro-marijuana political sentiment, for example, which may relax the perceived risk of marijuana use among the general population (Khatapoush and Hallfors, 2004; Hathaway et al., 2011; Cerdá et al., 2012). Loose legislative language has also put retail dispensaries and home cultivation operating in a gray area that may ultimately breed a marijuana supply source for nonpatients. For example, Pacula et al. (2015) finds that MMLs with legal protections for dispensaries lead to higher recreational marijuana use relative to MMLs without this supply channel. In Mark Anderson et al. (2013),

TABLE 8.
State Medical Marijuana Laws Effective as of May 2014

State	Date of legalization
Alaska	March 1999
Arizona	April 2011
California	November 1996
Colorado	June 2001
Connecticut	June 2012
Deleware	July 2011
DC	August 2010
Hawaii	January 2001
Illinois	January 2014
Maine	January 2000
Massachusetts	January 2013
Michigan	December 2008
Montana	November 2004
Nevada	October 2001
New Hampshire	August 2013
New Jersey	January 2010
New Mexico	July 2007
Oregon	December 1998
Rhode Island	January 2006
Vermont	July 2004
Washington	November 1998

Notes: Florida, Maryland, Minnesota, New York, North Dakota, Ohio, Pennsylvania, and West Virginia legalized marijuana after May 2014.

the supply of high-grade marijuana is said to increase over time after medical marijuana legalization is enacted, which in turn leads to reductions in the price of high-grade marijuana which presumably will increase demand.

Prior Literature on Caretaker Marijuana Use and Child Maltreatment

It has been shown that parents with substance-abuse afflictions have a higher risk of perpetrating child maltreatment, and particularly physical abuse and neglect (Famularo et al., 1992; Kelleher et al., 1994; Chaffin et al., 1996; Dube et al., 2001; Paxson and Waldfogel, 2002; Walsh et al., 2003; Barnard and McKeganey, 2004; Hohman et al., 2004; Cunningham and Finlay, 2013; Freisthler et al., 2017). Walsh et al. (2003) reports that parental substance abuse is associated with more than a two fold increase in the risk of exposure for child abuse in drug-using versus non-drug-using households. Not all of these studies explore the effect of substance abuse on maladaptive parenting by type of drug, and those that do make such distinctions primarily focus on the relationship between alcohol abuse (Famularo et al., 1992; Markowitz and Grossman, 1998; Walsh et al., 2003), cocaine use (Famularo et al., 1992), or methamphetamine use (Hohman et al., 2004; Cunningham and Finlay, 2013) and child maltreatment.

Few studies have explored the relationship between caretaker marijuana use and child maltreatment. For example, Freisthler et al. (2015) examines how current use and availability of marijuana correlates with caretaker physical abuse, and supervisory and physical neglect.[6] They collect telephone survey data from individuals in mid-sized cities in California in 2009. On average, they find that

[6]Child physical abuse was measured using the severe physical assault scale from Conflict Tactics Scale-Parent-Child Version (Straus et al., 1998). Neglect was measured using the Multidimensional Neglectful Behavior Scale (Kantor et al., 2003).

marijuana use is associated with higher levels of physical abuse, lower levels of physical neglect, and no difference in supervisory neglect. In a similarly designed study, Freisthler et al. (2017) finds that city-level and individual-level rates of marijuana use are positively correlated with child physical abuse and negatively correlated with child neglect. In Moller et al. (2011) and Douglas and Sullivan (2013), children residing in marijuana-cultivating homes were found to be no less healthy than children residing in non-cultivating homes. Specifically, there is no significant difference in the reporting of respiratory concerns, dermatological conditions, and ear infections between the children in marijuana cultivating-homes and the comparison group. To our knowledge, this study is the first to exploit temporal variation in the implementation of state-level MMLs to estimate the causal effects of marijuana use on child maltreatment.

Data

To examine the effect of marijuana use on maladaptive parenting we acquire data from the "Child Files" of the National Child Abuse and Neglect Data System (NCANDS), for federal fiscal years 2003 through 2014.[7]

[7]NCANDS collects data from all U.S states and the District of Columbia. The NCANDS Child Files files are distributed by the National Data Archive on Child Abuse and Neglect (NDACAN). States participate on a voluntary basis; the submitted data consist of all investigations or assessments of alleged child maltreatment that received a disposition in that reporting year. After conducting interviews with family members, the alleged victim, and others, the CPS agency makes a determination or finding concerning whether the child is a victim of abuse or neglect or is at risk of abuse or neglect. This determination is often called a disposition, and each State establishes specific dispositions and terminology (Child Maltreatment 2016, U.S. DHHS).

Our data are aggregated such that each observation corresponds to a unique report. A given report can correspond to multiple children (up to ten) and multiple perpetrators (up to three). Each report can include up to four maltreatment types per child. Data include report date, types of maltreatment and maltreatment dispositions listed on the report, child demographics, and services provided as a result of an investigation (e.g., family counseling or court services). Note that an alleged maltreater is only labeled a perpetrator if a corresponding maltreatment accusation is substantiated. Thus, for substantiated reports, data also include perpetrator information—such as number of perpetrators, race and age of perpetrator, and perpetrator risk factors (e.g., drug use, alcohol use, and intimate partner violence).

In our empirical analysis, we aggregate to monthly state-level observations on the number of maltreatment reports per 100,000 adults, from January 2003 through May 2014.We first examine the effect of marijuana use on the rates of total maltreatment reports, child-neglect reports, and child-physical-abuse reports.

In Table 9, we present NCANDS Child File descriptive statistics. On average, the rate of total investigated (or assessed) maltreatment reports is 68.9 per 100,000 adults. Our measure of total neglect is the sum of all reports that include neglect as a maltreatment type (whether the neglect was substantiated or not). Total physical abuse reports are similarly measured.[8] The average rate of reported

[8]Neglect and physical abuse are listed as a maltreatment type on 63 and 23 percent of reports in the sample, respectively.

neglect is roughly 42 per 100,000 adults and the average rate of reported physical abuse is roughly 21 per 100,000 adults. We also examine how marijuana use affects the rate of substantiation. A report is substantiated if at least one maltreatment allegation for any child on the report receives a determination of "substantiated," "indicated or reason to suspect," or "alternative response disposition-victim." On average, 17 reports per 100,000 adults are substantiated, and of those the rate of substantiated neglect is 11 per 100,000 and the rate of substantiated physical abuse is 5 per 100,000.

Among substantiated reports, roughly 50 percent correspond to to white perpetrator(s), perpetrators are white, 16 percent correspond to black perpetrator(s), and 10 percent correspond to Hispanic perpetrator(s), and one percent correspond to other or multiple races. Of substantiated reports with perpetrators of the same gender the majority of reports correspond to female perpetrator(s) versus male perpetrators—43 percent versus 32 percent. Last, over 60 percent of of substantiated reports correspond to an average perpetrator age between the ages of 25 and 44.

We obtain state-year population, and racial demographic, data from the National Cancer Institutes's Surveillance Epidemiology and End Results (Cancer-SEER) program. State-year median household income and state-month unemployment rate data come from the Bureau of Labor Statistics. Data from the Treatment Episode Data Set (TEDS) provide measures for other drug and alcohol

TABLE 9.
National Child Abuse and Neglect Data System Child File
Descriptive Statistics, 2003-2014

	Total (1)	No MML (2)	MML (3)
Maltreatment report rate per 100,000 adults	68.916	71.148	65.695
	(25.034)	(28.380)	(18.749)
Neglect allegation rate per 100,000 adults	41.787	41.546	42.134
	(22.960)	(25.376)	(18.938)
Physical abuse allegation rate per 100,000 adults	20.813	20.989	20.558
	(9.318)	(10.197)	(7.873)
Report substantiation rate per 100,000 adults	16.910	17.186	16.512
	(8.801)	(9.007)	(8.481)
Neglect substantiation rate per 100,000 adults	11.403	11.018	11.960
	(7.509)	(7.051)	(8.095)
Physical abuse substantiation rate per 100,000 adults	4.660	4.742	4.541
	(2.678)	(2.796)	(2.494)
Fraction of substantiated reports, white perpetrator(s)	0.514	0.550	0.465
	(0.193)	(0.167)	(0.214)
Fraction of substantiated reports, black perpetrator(s)	0.160	0.170	0.146
	(0.132)	(0.117)	(0.150)
Fraction of substantiated reports, hispanic perpetrator(s)	0.102	0.068	0.149
	(0.114)	(0.080)	(0.134)
Fraction of substantiated reports, other race perpetrator(s)	0.060	0.045	0.081
	(0.119)	(0.091)	(0.146)
Fraction of substantiated reports, multiple race perpetrators	0.048	0.039	0.060
	(0.043)	(0.041)	(0.043)
Fraction of substantiated reports, female perpetrator(s)	0.430	0.415	0.451
	(0.107)	(0.108)	(0.103)
Fraction of substantiated reports, male perpetrator(s)	0.318	0.328	0.304
	(0.128)	(0.132)	(0.121)
Fraction of substantiated reports, average perpetrator age <25	0.203	0.210	0.194
	(0.057)	(0.058)	(0.055)
Fraction of substantiated reports, average perpetrator age 25-44	0.622	0.602	0.650
	(0.095)	(0.107)	(0.067)
Fraction of substantiated reports, average perpetrator age >44	0.175	0.188	0.156
	(0.102)	(0.124)	(0.052)
Observations	6,478	3,826	2,652

Notes: The data source is NCANDS child file data maltreatment reports filed from January 2003-June 2014. All perpetrator information is missing from Georgia, and racial information is missing from Pennsylvania. Racial, gender, and age group fractions do not add to one as some substantiated reports are missing select perpetrator characteristic information.

use. Last, state-level data on MML legislation effective dates were collected from ProCon.org (2013).

Emprical Analysis

Method and Identification

In order to estimate the effect of marijuana use on maltreatment report rates, we adopt a difference-in-differences design to explore whether the rate of child maltreatment reports changes systematically with state-level MMLs. Consequently, we estimate

$$Maltreatment_{smy} = \beta_0 + \beta_1 1(MML_{smy}) + \delta X_{smy} + \alpha_s + \lambda_m + \gamma_y + t_{smy} + u_{smy}, \tag{3.1}$$

where y_{smy} is the rate of maltreatment reports per 100,000 adults in state s in month m of year y. Specifically, we consider total reports, neglect reports, physical abuse reports, or substantiated reports. $1(MML_{smy})$ is an indicator variable, equal to one if state s has an MML in effect, and zero otherwise. State, year, and month fixed effects—α_s, λ_m, and γ_y—are included in order to control for time-invariant heterogeneity across states, and time-varying and seasonal shocks to adoption outcomes that are constant across states. We control for observable time-varying state-level heterogeneity in X_{smy}, which includes controls for racial composition, unemployment rate, and median household income, and a dummy variable for whether a state had a marijuana decriminalization law in place.[9] In

[9]A state is said to have decriminalized marijuana when the state reduces the penalty for possessing a small amount of marijuana to a fine rather than imprisonment. During the period under study, the decriminalization indicator captures four legislative changes: Connecticut (2011),

alternative specifications, we include measures of drug use, such as the fraction of total substance abuse treatment admissions that include methamphetamine abuse, the fraction that include heroin abuse, and the fraction that include alcohol abuse. (The sample sizes will be smaller when these controls are included, as TEDS data for substance abuse treatment admissions are missing for some state-year observations.) In addition to state and time fixed effects, we include state-specific linear time trends, t_{smy} to capture time-varying unobservables that evolve at a constant rate. The inclusion of state-specific linear time trends allows us to interpret the parameter of interest, $\hat{\beta}_1$, as the average deviation from state-specific trends coincident with treatment. Last, we estimate the error term, u_{smy}, allowing for clustering at the state level.

The main identifying assumption of our difference-in-differences approach is that maltreatment report rates in treatment states would have changed in a way similar to report rates in control states in the absence of the legislative change—that maltreatment in treated and untreated states is trending similarly. In later analysis, we estimate our primary specification including leading indicators, and find no evidence of a violation of our identifying assumption.

Massachusetts (2009), Rhode Island (2013), and Vermont (2013). The remaining 12 states passed a decriminalization law prior to 2003.

Results

In Table 10, we present the estimates for the effect of medical marijuana laws on the adult population rate of maltreatment reports—namely total reports, reports including neglect as a type of maltreatment, and reports including physical abuse as a type of maltreatment. In Table 10, each panel corresponds to a different specification and each column corresponds to a different outcome category. Specifically, in columns (1) through (3), we report the estimated effect of treatment on the rate of maltreatment reports, neglect reports, and physical abuse reports respectively.

The results we depict in Panel A of Table 10 are the estimated effects of MMLs on each of the three possible outcomes controlling for month-, year-, and state-fixed effects. In Panel B we add state-specific linear trends, and in Panel C, we add time-varying state-level controls. In specifications that include a control for differences in state-specific trends of maltreatment, the treatment effect on the population rate of physical abuse reports is negative and statistically significant at the five-percent level. The sizable difference between the estimates presented in Panel A and those presented in panels B and C suggest that accounting for state trends is important in assessing the impact of medical marijuana legalization. The results presented in Panel C, our preferred specification corresponding to Equation (1), indicate that, on average, medical marijuana legalization significantly decreases the rate of reports indicating any physical abuse by 2.23 per 100,000

adults. Relative to the pre-treatment mean, this represents a 10.4 percent decline. Our results further indicate that marijuana use does not increase the rate of total maltreatment

TABLE 10.
Effects of Medical Marijuana Laws on Maltreatment Report Rates per 100,000 Adult Population

	Total Maltreatment Reports (1)	Neglect Reports (2)	Physical Abuse Reports (3)
Panel A: State, Month, and Year Fixed Effects			
MML	-0.390	1.795	1.482
	(3.639)	(3.619)	(2.137)
Observations	6,478	6,478	6,478
Pre-Treatment Mean	67.420	42.693	21.521
Impact (%) at Pre-Treatment Mean	-0.6	4.2	6.9
Effect Size	0.016	0.078	0.159
Panel B: Fixed Effects + State-Specific Linear Trends			
MML	-3.177	-1.205	-2.570**
	(3.426)	(2.771)	(0.976)
Observations	6,478	6,478	6,478
Pre-Treatment Mean	67.420	42.693	21.521
Impact (%) at Pre-Treatment Mean	-4.7	-2.8	-11.9
Effect Size	0.127	0.052	0.276
Panel C: Fixed effects + state-specific linear time trends + time varying controls			
MML	-2.343	-0.902	-2.257**
	(3.110)	(2.701)	(0.882)
Observations	6,478	6,478	6,478
Pre-Treatment Mean	67.420	42.693	21.521
Impact (%) at Pre-Treatment Mean	-3.5	-2.1	-10.5
Effect Size	0.094	0.039	0.242
Panel D: Fixed effects + state-specific linear time trends + time varying controls + drug use controls			
MML	-1.924	-1.037	-2.344***
	(3.080)	(2.543)	(0.847)
Observations	6,256	6,256	6,256
Pre-Treatment Mean	67.420	42.693	21.521
Impact (%) at Pre-Treatment Mean	-2.9	-2.4	-10.9
Effect Size	0.077	0.045	0.252

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. NCANDS child file data from January 2003-June 2014. Each row corresponds to a separate regression. All specifications control for state, year and month fixed effects. Time varying state-level controls include population demographics, unemployment rate, household median income, and an indicator for marijuana decriminalization. Standard errors adjust for clustering at the state level.

reports or reports indicating any neglect. In fact, treatment is associated with a decrease in the rate of total maltreatment reports of similar magnitude to the effect estimated for rates of physical abuse reports. However, the effect on rates of total maltreatment reports is statistically indistinguishable from zero.

It is possible that MMLs do not affect actual perpetration of child maltreatment, but instead affect an individual's propensity to report suspected maltreatment due to marijuana use.[10] However, if this were the case we might anticipate a larger treatment effect associated with the rate of neglect reporting (relative to the rate of physical abuse reporting) as some CPS agencies classify "child exposure to drugs/drugs in the presence of a child" as neglect. Note that some states instead classify this as "Other."[11]

There are two primary mechanisms through which we expect increased access to marijuana to decrease rates of child physical abuse. First, it may be that marijuana use directly decreases a caretaker's propensity to physically abuse their child. For example, Thurstone et al. (2013) finds that some parents report using marijuana in order to prevent themselves from hitting their child. Additionally, some studies have shown that marijuana use is inversely related to aggressive

[10]Primary reporting sources include education personnel, law enforcement and legal personnel, and nonprofessional sources such as friends and family.

[11]Although not reported, we conduct similar analysis on the effect of marijuana use on the adult population rate of reports indicating other maltreatment, and find a treatment effect that is statistically insignificant, and small in magnitude. Thus, it is unlikely that the results we present in Table 10 are driven by changes in reporting behavior.

behavior and the perpetration of intimate partner violence (Smith et al., 2012; Stuart et al., 2013; Perna et al., 2016).

Second, it may be that marijuana use is a substitute for harder drug or alcohol use, both of which have been shown to increase child abuse. Recent research has demonstrated substitution between marijuana and alcohol, heroin, and opioids (Mark Anderson et al., 2013; Chu, 2015; Powell et al., 2015; Ozluk, 2017). Thus, our estimates may be driven by a reduction in the use of other substances that are strong risk factors for the perpetration of child maltreatment. As the data do not indicate if specific substances were present with respect to maltreatment reports, we cannot separately identify these two potential mechanisms. However, in Panel D of Table 10 we re-estimate our preferred specification with the inclusion of drug-use controls, including the state-level fractions of total substance abuse treatment admissions that include methamphetamine use, heroin use, and alcohol use.[12] The inclusion of these controls does not substantively change our baseline results, and thus there is no evidence suggesting that a substitutionary mechanism is the primary channel through which MML change child welfare.

We also consider lead and lag effects of MMLs. Specifically, we add separate indicators for one-to-four years prior to treatment, five-or-more years prior to treatment, the year of treatment, one-to-four years post-treatment, and five-or-more years post treatment. One-year prior to treatment is the excluded category.

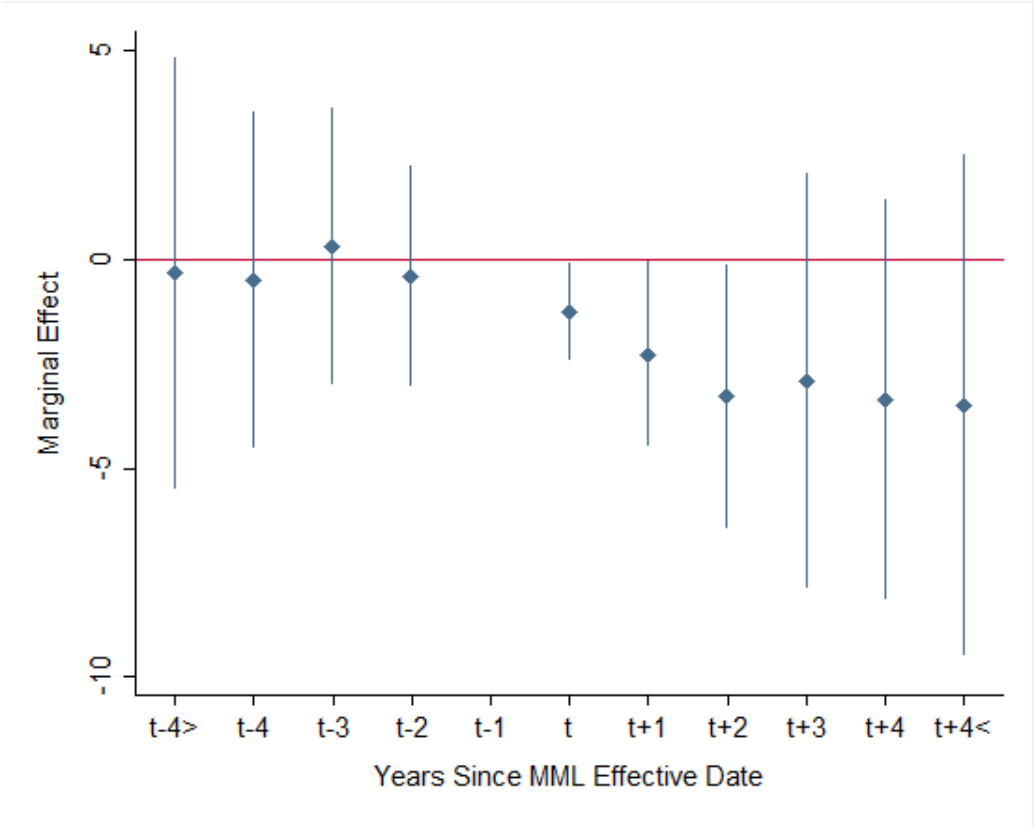
[12]When we re-estimate the analysis conducted in Panel C omitting the 222 state-year observations missing from the drug use treatment admissions data, our results are qualitatively similar to those presented in Panel C.

Given that our estimates presented in Table 10 indicate treatment only statistically significantly affects the population rate of physical abuse reporting, this event-study analysis will focus on this outcome, which we summarize in Figure 12. There is no evidence that systematic differences in pre-treatment trends across treatment and control states are driving our previously estimated result, as lead indicators are close to zero. Treatment not only significantly decreases physical abuse reporting rates in the first year of treatment, but also the effect persists in the years subsequent to the law.

Next, we examine how increased availability of marijuana may influence the rate of substantiated maltreatment reports. Unlike a maltreatment reports, which can be filed by any individual, substantiated reports have been vetted by the responsible agency and the claim of maltreatment has thereby been supported by a child protective services caseworker following an investigation or assessment. For this analysis, we re-estimate Equation (1) on rates of substantiation for all reports, neglect, and physical abuse, which we summarize in columns 1 through 3 of Table 11.

Although treatment does not appear to significantly decrease the average rate of overall substantiated reports, there are significant declines in the average rate of any substantiated physical abuse coincident with MML, by 0.43 per 100,000 adults, or 8.9 percent relative to the mean. This is similar in magnitude to that of MML

FIGURE 12.
MML on the Rate of Any Physical Abuse Report per 100,000 Adults,
Before and After Effective Legislation Years



Notes: The figures display coefficient estimates, and their 95% confidence intervals for the leading indicators and lagged treatment, from an OLS regression, accounting for state, month, and year fixed effects, state specific linear time trends, and the aforementioned time-varying state-specific covariates including percent black, percent white, unemployment rate, median household income, and an indicator for marijuana decriminalization. Standard errors are adjusted for clustering at the state-level.

on the rate of reported physical abuse (Table 10, Panel C), suggesting that the change in reports represents real declines in child abuse.

Among substantiated reports of physical abuse, in Table 12 we next examine how treatment affects the rate at which drug use, alcohol use, or intimate partner

TABLE 11.
Effects of Medical Marijuana Laws on Maltreatment Report Rates per 100,000 Adult Population, Substantiated Reports

	Total Substantiated Reports (1)	Substantiated Neglect Reports (2)	Substantiated Physical Abuse Reports (3)
MML	-0.686	0.133	-0.398*
	(0.975)	(0.859)	(0.224)
Observations	6,478	6,478	6,478
Pre-Treatment Mean	17.502	13.031	4.849
Impact (%) at Pre-Treatment Mean	-3.9	1.0	-8.2
Effect Size	0.078	0.018	0.149
State and time fixed effects	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. NCANDS child file data from January 2003 - June 2014. Each row and column correspond to a separate OLS regression. All specifications control for state, year and month fixed effects, and state-specific linear time trends. Time varying state-level controls include population demographics, unemployment rate, household median income, and a dummy for marijuana decriminalization. Standard errors adjust for clustering at the state level.

violence (IPV) are indicated as a maltreatment risk factor.[13] This analysis will provide further insight as to whether the substitutability between marijuana use and other substance abuse plays a role in explaining the estimated reductions in child abuse rates summarized in tables 10 and 11. We find that MMLs decrease the rate of indicating caretaker drug use as a risk factor on an abuse report by 29 percent at the pre-treatment mean (Column 1).

The reductions in the rate of drug use as a reported risk factor could reflect a relaxation in caseworker perceptions or regarding the maltreatment risk associated

[13]Colorado, Connecticut, Kansas, Louisiana, Montana, North Carolina, Virginia, Vermont, Nevada, West Virginia, Tennessee, and Oklahoma do not report intimate partner violence as a risk factor. Idaho, Iowa, Massachusetts, Illinois, and New York do not report drug or alcohol use. Indiana and Alabama do not report alcohol use.

with (medical) marijuana presence/use in child physical abuse cases. However, the legal status of a substance does not determine risk factors in abuse classifications, and caseworkers can still remove children from their caretakers with marijuana in the household being indicated as a contributing factor.[14] Thus, with no change in procedures, we are not inclined to attribute the evident movement reported risk factors in Table 12 to changes in reporting.

Alternatively, it could be that marijuana use not only decreases the propensity to perpetrate child abuse, but also serves as a substitute for hard drug use which has been shown to increase the propensity to perpetrate physical abuse (Kelleher et al., 1994; Chaffin et al., 1996; Walsh et al., 2003; Hohman et al., 2004; Cunningham and Finlay, 2013; Freisthler et al., 2017). Nonetheless, we do not similarly find a statistically or economically significant decline in the rate of indication of alcohol use (Column 2), despite the anticipated substitution effects leading to declines in alcohol use Mark Anderson et al. (2013). As with previous results, without indication of the specific type of drugs present during investigations of maltreatment, we can not speak directly to the causal mechanism underlying these results.

Last, in Column (3) of Table 12, we explore how treatment affects rates of indicated intimate partner violence. Despite the significant association between marijuana use and IPV (Smith et al., 2012; Stuart et al., 2013), our estimates are

[14]Moreover, most agencies define illegal drug use by parents using federal scheduling guidelines. See `http://flcalliance.org/resources/state-profiles/` for examples, state CPS policies, and procedures.

TABLE 12.
Effects of Medical Marijuana Laws on Caretaker Risk Factors, Substantiated Physical Abuse Reports

	Drug Use	Alcohol Use	Intimate Partner Violence
	(1)	(2)	(3)
MML	-0.132***	-0.013	-0.054
	(0.047)	(0.071)	(0.155)
Observations	4,405	3,550	4,513
Pre-Treatment Mean	0.462	0.463	0.901
Impact (%) at Pre-Treatment Mean	-28.6	-2.8	-6.0
Effect Size	0.170	0.024	0.056
State and time fixed effects	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. NCANDS child file data from January 2003-June 2014. Each row and column correspond to a separate OLS regression. All specifications control for state, year and month fixed effects, and state-specific linear time trends. Time varying state-level controls include population demographics, unemployment rate, household median income, and a dummy for marijuana decriminalization. Standard errors adjust for clustering at the state level.

statistically indistinguishable from zero.[15] When we conduct the same analysis for the subset of substantiated neglect reports, in Table 13, results are qualitatively similar to those reported in Table 12, and statistically insignificant. The exact designation of a caretaker risk factor is varied and unknown across state CPS agencies, and much is left to the discretion of the individual caseworker; therefore we caution against a conclusive interpretation of the causal mechanism underlying the results presented in tables 12 and 13. Nonetheless, the estimates present in tables 12 and 13 do provide further evidence that the reductions in child physical

[15]The estimated effect of marijuana availability on the rate of physical abuse substantiation for each of the subsets of states that report these relevant caregiver risk factors are quantitatively similar to the estimates reported in Table 11, Column (3).

abuse following marijuana liberalization are not likely driven by substitution away from alcohol use.

TABLE 13.
Effects of Medical Marijuana Laws on Caretaker Risk Factors, Substantiated Neglect Reports

	Drug Use	Alcohol Use	Intimate Partner Violence
	(1)	(2)	(3)
MML	-0.636	-0.142	0.072
	(0.492)	(0.196)	(0.391)
Observations	4,405	3,640	4,513
Pre-Treatment Mean	2.476	1.876	1.651
Impact (%) at Pre-Treatment Mean	-25.7	-7.6	4.4
Effect Size	0.262	0.073	0.034
State and time fixed effects	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. NCANDS child file data from January 2003-June 2014. Each row and column correspond to a separate OLS regression. All specifications control for state, year and month fixed effects, and state-specific linear time trends. Time varying state-level controls include population demographics, unemployment rate, household median income, and a dummy for marijuana decriminalization. Standard errors adjust for clustering at the state level.

Subgroup Analysis

We next explore how treatment differentially affects maladaptive parenting across different population gender-, age- and race-specific subgroups. If the report is unsubstantiated, the accused maltreater's race is not provided, but the races of the children on the report are provided. Thus, for analysis of the effect of MML on rates of maltreatment reports (whether substantiated or not), we designate race

based on the race of the children listed on the report.[16] In Table 14, we summarize the estimates corresponding to our racial heterogeneity analysis. In panels A and B, we report the estimated effect of MML on the rates of maltreatment reports (whether substantiated or not) per 100,000 adults for white and black racial subgroups, respectively. We see that MML significantly decreases the rates of reporting any physical abuse for both white and black caretakers (see Column 3 of panels A and B). Specifically, increased marijuana availability decreases the rate of any physical-abuse reporting for white caretakers by 1.3 per 100,000 white adults, and by 3.4 per 100,000 black adults for black caretakers. Although the magnitude of the estimated treatment effect is larger for black caretaker's the underlying differences in baseline rates of physical abuse implies that the impact at the pre-treatment mean is larger for white caretakers—a 13.5 percent decrease among white adults versus a 9.2 percent decrease among black adults.

In panels C and D of Table 14, we report the estimated effect of MML on the rates of substantiated neglect (Column 2), any substantiated physical abuse (Column 3), and any substantiated maltreatment (Column 4), for white and black perpetrators respectively. (For our subgroup designations, we use information on the actual reported race of the perpetrator.) In this case, the impact of treatment on the population rate of physical abuse substantiation for white and

[16]If all the children on a report are of a certain subgroup the report is designated as part of that subgroup. Reports including children of different races are placed in a "mixed" race category.

black populations are similar to the respective impact sizes of treatment on the population rates of physical abuse reporting (panels A and B).

TABLE 14.
Effects of Medical Marijuana Laws on Maltreatment Rates per 100,000, by race

	Total Maltreatment Reports (1)	Neglect Reports (2)	Physical Abuse Reports (3)	Total Substantiated Reports (4)
Panel A: White				
MML	-1.653	-0.610	-1.362*	-0.259
	(1.748)	(1.207)	(0.708)	(0.363)
Observations	6,223	6,223	6,223	6,223
Pre-Treatment Mean	31.608	19.744	9.916	7.302
Impact (%) at Pre-Treatment Mean	-5.2	-3.1	-13.7	-3.5
Effect Size	0.080	0.043	0.185	0.045
Panel A: Black				
MML	-1.982	-2.722	-3.484*	-0.576
	(4.886)	(2.995)	(1.734)	(1.446)
Observations	6,223	6,223	6,223	6,223
Pre-Treatment Mean	108.829	66.898	36.378	28.878
Impact (%) at Pre-Treatment Mean	-1.8	-4.1	-9.6	-2.0
Effect Size	0.037	0.069	0.167	0.028
Panel A: White, Substantiated Reports				
MML		0.187	-0.244*	-0.237
		(0.294)	(0.135)	(0.382)
Observations		6,361	6,361	6,361
Pre-Treatment Mean		5.695	2.006	7.629
Impact (%) at Pre-Treatment Mean		3.3	-12.2	-3.1
Effect Size		0.044	0.132	0.040
Panel A: Black, Substantiated Reports				
MML		0.180	-0.894	-1.112
		(1.382)	(0.589)	(1.596)
Observations		6,361	6,361	6,361
Pre-Treatment Mean		22.470	9.459	30.908
Impact (%) at Pre-Treatment Mean		0.8	-9.5	-3.6
Effect Size		0.011	0.128	0.051
State and time fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. NCANDS child file data from January 2003-June 2014. Each row and column correspond to a separate OLS regression. All specifications control for state, year and month fixed effects, and state-specific linear time trends. Time varying state-level controls include population demographics, unemployment rate, household median income, and a dummy for marijuana decriminalization. Standard errors adjust for clustering at the state level. Pennsylvania and Georgia are omitted from this analysis as perpetrator race is not reported.

Furthermore, the difference in the magnitude of the treatment effect for white and black caretakers, 0.246 and 0.898 respectively, is roughly the same size as the difference in the treatment effect on abuse reporting. However, the effect of MML on the rate of substantiated physical abuse reports per 100,000 black adults is statistically insignificant.

In panels C and D of Table 14, we report the estimated effect of MML on the rates of substantiated neglect (Column 2), any substantiated physical abuse (Column 3), and any substantiated maltreatment (Column 4), for white and black perpetrators respectively. (For our subgroup designations, we use information on the actual reported race of the perpetrator.)In this case, the impact of treatment on the population rate of physical abuse substantiation for white and black populations are similar to the respective impact sizes of treatment on the population rates of physical abuse reporting (panels A and B). Furthermore, the difference in the magnitude of the treatment effect for white and black caretakers, 0.246 and 0.898 respectively, is roughly the same size as the difference in the treatment effect on abuse reporting. However, the effect of MML on the rate of substantiated physical abuse reports per 100,000 black adults is statistically insignificant.

In panels A and B of Table 15, we report the estimated effect of MML on the rates of any substantiated maltreatment (Column 1), any substantiated neglect (Column 2), and any substantiated physical abuse (Column 3), for female

and male perpetrators respectively. (For this analysis we restrict the sample to substantiated reports corresponding to perpetrator(s) of a single gender; note that over 80 percent of reports that list perpetrators of different genders correspond to the parents of the child.) Our results indicate that not only do reductions in the rate of physical abuse rates appear to be driven primarily by reductions in male perpetration (Column 3), but also for male perpetrators treatment significantly reduces the overall rate of any substantiated maltreatment by about 0.9 reports per 100,000 adult males (Column 1). As summarized in columns (1) and (2), the impact of MMLs at the pre-treatment mean is more than double for male perpetrators than for female perpetrators—about 9-10 percent versus 4 percent.

The results presented in Table 15 are consistent with previous studies that have looked at the differential effects of medical marijuana laws on general population marijuana use, and other related outcomes (Mark Anderson et al., 2013; Chu, 2014; Wen et al., 2015).

Last, we conduct similar analysis across the age of the perpetrator—we create three age groups less-than-25, 25-to-44, and greater-than-44—the results of this analysis are reported in Table 16.[17] As the age of the accused on a report is only available if a report is substantiated, we consider the potential heterogeneity by age in the effect of MML on the adult population rate of report substantiation,

[17]As multiple perpetrators may appear on a single report, we use the average perpetrator age for our designation. The results of this analysis are not sensitive to the specific age cutoffs.

TABLE 15.
Effects of Medical Marijuana Laws on Maltreatment Rates per 100,000, by Gender

	Total Substantiated Reports (1)	Substantiated Neglect Reports (2)	Substantiated Physical Abuse Reports (3)
Panel A: Female			
MML	-0.721	0.156	-0.151
	(1.177)	(1.014)	(0.179)
Observations	6,361	6,361	6,361
Pre-Treatment Mean	16.760	13.128	3.061
Impact (%) at Pre-Treatment Mean	-4.3	1.2	-4.9
Effect Size	0.085	0.021	0.074
Panel B: Male			
MML	-0.902**	-0.129	-0.324**
	(0.427)	(0.312)	(0.145)
Observations	6,361	6,361	6,361
Pre-Treatment Mean	10.498	5.013	3.093
Impact (%) at Pre-Treatment Mean	-8.6	-2.6	-10.5
Effect Size	0.133	0.034	0.185
State and time fixed effects	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. NCANDS child file data from January 2003-June 2014. Each row and column correspond to a separate OLS regression. All specifications control for state, year and month fixed effects, and state-specific linear time trends. Time varying state-level controls include population demographics, unemployment rate, household median income, and a dummy for marijuana decriminalization. Standard errors adjust for clustering at the state level.

substantiation of neglect, and substantiation of physical abuse. The results we summarize in Column (3) suggest that medical marijuana legalization has the largest effect on rate of physical abuse perpetration among adults ages 25-44. As before, our results are expected and consistent with the literature that has looked at the effect of MMLs on marijuana use and related outcomes.

Conclusion

Through the exploitation of state-level variation in the timing of medical marijuana legalization (MML), we employ a difference-in-differences approach to estimating the effects of increased marijuana availability on rates of child maltreatment. Specifically, we estimate the perpetration of child abuse and neglect. Using data on U.S. child-maltreatment reports available from January 2003 to May 2014, we find evidence that suggests that MML decreases the rates of reported and substantiated physical abuse
by 10 percent and 9 percent, respectively. We do not find evidence that MML has a significant effect on overall maltreatment or child-neglect rates. However, we do find that among substantiated physical-abuse cases, medical marijuana legalization decreases the rates at which CPS agents report drug use as a caretaker risk factor. Our results would also be consistent with the legalization of marijuana for medical purposes changing CPS perceptions of marijuana use as a contributing factor to the risk of child abuse. Although our findings differ from the correlational results of previous case studies (e.g., Freisthler et al. (2015)), we are the first to exploit identifying variation that establishes a control group of comparators in a difference-in-differences approach, using national data on actual child maltreatment reports, to estimate the effect of MML on child abuse and neglect.

Without a measurable first-stage response—recall that we have no direct measure of marijuana use among caregivers—we cannot weight the reduced-form

TABLE 16.
Effects of Medical Marijuana Laws on Maltreatment Rates per 100,000, by Age

	Total Substantiated Reports (1)	Substantiated Neglect Reports (2)	Substantiated Physical Abuse Reports (3)
Panel A: Ages 18-24			
MML	-0.367	1.025	-0.194
	(1.293)	(1.161)	(0.285)
Observations	6,361	6,361	6,361
Pre-Treatment Mean	21.135	15.811	4.309
Impact (%) at Pre-Treatment Mean	-1.7	6.5	-4.5
Effect Size	0.029	0.104	0.056
Panel B: Ages 25-44			
MML	-0.890	0.329	-0.799*
	(1.881)	(1.731)	(0.444)
Observations	6,361	6,361	6,361
Pre-Treatment Mean	31.496	24.030	8.941
Impact (%) at Pre-Treatment Mean	-2.8	1.4	-8.9
Effect Size	0.055	0.022	0.154
Panel C:Ages 44+			
MML	-0.676	-0.309	-0.203
	(0.444)	(0.282)	(0.127)
Observations	6,361	6,361	6,361
Pre-Treatment Mean	5.977	4.032	1.915
Impact (%) at Pre-Treatment Mean	-11.3	-7.7	-10.6
Effect Size	0.140	0.089	0.125
State and time fixed effects	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. NCANDS child file data from January 2003-June 2014. Each row and column correspond to a separate OLS regression. All specifications control for state, year and month fixed effects, and state-specific linear time trends. Time varying state-level controls include population demographics, unemployment rate, household median income, and a dummy for marijuana decriminalization. Standard errors adjust for clustering at the state level.

parameter by the responsiveness of marijuana use to medical legalization. Existing research has shown that parental use of other illicit drugs and alcohol increase the prevalence of child physical abuse and neglect. Thus, to the extent marijuana

serves as a substitute for these other substances (Mark Anderson et al., 2013; Chu, 2015), we cannot separately identify whether the evident improvements in child welfare coincident with MML are chiefly driven by this substitution, or if marijuana use directly decreases the perpetration of abuse and neglect. Regardless, moving toward richer understandings of the relationship between substance use and child maltreatment is key to best responding to any relaxation of barriers to substance use.

Although we can't speak directly to the effect of general marijuana use on the perpetration of child maltreatment, as studies have shown that MMLs increase adult marijuana use among nonpatients, we anticipate that policies which further increase marijuana accessibility will lead to qualitatively similar child maltreatment outcomes. As with previous studies that have explored how MMLs affect marijuana use and related health outcomes, we expect such policies to have the greatest impacts on males and adults over the age of 25. In addition, as there are currently 20 states without an MML in effect, our study speaks directly to a potential positive externality of such a law.

CHAPTER IV

THE EFFECT OF ANTI-BULLYING LAWS ON SUICIDALITY AND DRUG USE

Introduction

In 2013, over 5 million students, ages 12-18, reported being bullied during the school year (NCES, 2015).[1] Bullying is most commonly defined as "unwanted aggressive behavior among school aged children that involves a real or perceived power imbalance" (U.S. Department of Health and Human Services, 2014a).[2] Bullying behaviors can include, but are not limited to, name calling, spreading rumors, intentional property damage, and inflicting physical harm (NCES, 2015).

Furthermore, studies have established a link between bullying and poor physical health, mental health, and social connectedness (Allison et al., 2009; Duncan, 1999; Gini and Pozzoli, 2013; Nansel et al., 2001, 2004; Smith et al., 2004). Specifically, bullying has been linked to attempted and completed suicide (Hinduja and Patchin, 2010; Kaltiala-Heino et al., 2010; Kim et al., 2005; Birkett et al., 2009; Klomek et al., 2010). In addition, both victims and perpetrators of bullying have been shown to be more likely to use illegal drugs and abuse alcohol (Farrington

[1]The NCES report can be found at: `http://nces.ed.gov/pubs2015/2015056.pdf` (accessed June 1, 2016).

[2]The (U.S. Department of Health and Human Services definition can be found at: `http://mchb.hrsa.gov/chusa14/health-status-behaviors/adolescents/bullying.html` (accessed June 1, 2016)

and Ttofi, 2011; Luk et al., 2010; Nansel et al., 2004; Tharp-Taylor et al., 2009; Niemelä et al., 2011). Given the prevalence of bullying and its association with negative health outcomes, currently, all 50 states and the District of Columbia have passed antibullying laws (ABLs) mandating specific school-level interventions.

Studies have linked ABLs with reductions in bullying behavior and reported bullying incidents—Hatzenbuehler et al. (2015); Sabia and Bass (2017)—however, ABLs are designed to reduce not only bullying behavior, but also the emotional trauma associated with bullying. As such, this study is the first to examine the causal effect of antibullying laws on teen suicidality and illicit drug use. We exploit the state-year variation in the passage of ABLs using a difference-in-differences empirical approach, and find that, among male adolescents, ABLs lead to modest reductions in reported suicidal attempts and ideation, drug use, and accidental drug overdose. However, when we look into the efficacy of specific ABL components, we find that a mandated reporting system and/or investigation process leads to increases in drug use and suicidality, with impact magnitudes ranging from 8% to 30% at the pre-treatment mean. These results suggest that, across component composition, ABLs heterogenously impact adolescent suicidality and drug use, and more labor intensive component mandates actual exacerbate bullying-related mental health outcomes.

In Section 2 we detail the ABL components of interests as well as the related literature. In Section 3, we describe our data sources, and in Section 4, we develop

our empirical specification. In section 5, we establish the impact of ABLs on self-reported teen suicidal and drug use behaviors, and then turn to administrative data to explore how these laws impact completed suicide and illicit drug overdose rates. In Section 6, we conclude with a discussion on the implications of our results, and suggestions for future work.

Background

In the aftermath of the Columbine high school shooting in Littleton, Colorado, and in response to a local bullying-related suicide, Georgia became the first state to enact an antibullying law in 1999 (U.S. Department of Education (DOE), 2011).[3] As of 2015, all 50 states have passed antibullying legislation; most antibullying laws were enacted in the mid- to late- 2000s. During this time, there was increased pressure on governments for more effective prevention and reduction in bullying (U.S. DOE, 201l). In response to this pressure, in August 2010, the U.S. Department of Education (DOE) and the U.S. Department of Health and Human Services co-hosted the first Federal Partners in Bullying Prevention Summit. In order to categorize the scope, rigor, and quality of state antibullying legislation, the DOE drafted a study which organized the legislative provisions in antibullying laws into 16 components. Although the DOE study examines the extent to which state

[3]The U.S. Department of Education Report can be found at: `https://www2.ed.gov/rschstat/eval/bullying/state-bullying-laws/state-bullying-laws.pdf` (accessed October 6, 2015)

laws address these components, the study does not evaluate the impact and efficacy of antibullying legislation or specific legislative components.

The 16 DOE-identified components fall into 4 specified categories: (i)*Definitions of Terms*, (ii) *District Policy Development and Review*, (iii) *School District Components*, and (iv) *Additional District Policy Components*. The extent of component coverage in antibullying legislation ranges from 2 components to 16 components.[4] As of the present, all states mandate/recommend the development and implementation of school district-level bullying policies; however, only 29 states have specified a deadline by which these policies must be adopted by school districts. Table 17 chronologically summarizes, by state, the school district deadline date by which ABL policies must be implemented. For states without a school district deadline, the ABL's effective date is instead listed.

As states vary widely in component coverage, the emphasis of our analysis will be on the heterogenous effects of specific ABL components on suicidality and substance use. Each ABL can have up to 16 DOE specified components, we will focus on a subset of school district policy components. The first component, *Reporting*, mandates school districts to implement a system that would allow individuals to anonymously report suspected bullying. More than half of state ABLs with a reporting mandate not only require schools to establish a reporting procedure, but also encourage school personnel to report witnessed acts of bullying.

[4]For a full list and detailed explanation of each of the 16 ABL components, see U.S. DOE (2011), Exhibits 1 and 15.

Next, an *Investigations* mandate requires schools to implement a procedure for the prompt investigation of any reported act of bullying. Third, we have the *Training and Prevention* component which requires schools to establish "Bullying Prevention Task forces", and/or implement training programs for school personnel. For instance, New Jersey's ABL requires each public school "provide training on the school district's harassment, intimidation or bullying policies to school employees and volunteers who have significant contact with students" (*N.J. Stat. Ann.* §18A:37-15 2002). In New Jersey, in 2012, about half of the two million dollars school districts spent to implement ABL policies were reportedly spent on implementing teacher and staff training (The Press of Atlantic City, 2012).[5]

Last, we look at the components *Written Records & Transperancy*, together, these provisions require school districts to keep records of reported bullying altercations, and report the annual number of bullying-related incidents to the state board of education, and in a few instances, to the public. For example, the state of Maryland requires the state DOE to prepare and distribute–to all public schools–a standardized reporting form for documenting reported bullying incidents. The form contains information on the bully, victim, physical injuries, resulting school absences, etc. The law also mandates that schools return completed forms to the county board of education, who then provides annual summaries of compiled data to the state board (*Md. Code Ann., Educ.* §7-424). Of the aforementioned

[5]This article can be found at `http://www.pressofatlanticcity.com/news/breaking/schooldistricts-say-state-s-antibullying-law-costs-at/article_8474d72c-6989-11e1-b18d-0019bb2963f4.html` (accessed April 1, 2016).

components, a *Transperancy* component is relatively rare in state ABLs. We restrict our attention to these specific components, not only because they are most likely to impact marginal bullying decisions, but also they are arguably the most expensive components to implement in terms of school district finances, resources, and time. Table 17 lists ABL component coverage by state.

Few studies have explored the impact of ABLs on bullying behavior, and even fewer examine the impact of ABLs on bullying related outcomes. Sabia and Bass (2017), is the first study to find arguably causal evidence that ABLs lead to a reduction in bullying behavior. Specifically, by exploiting a difference-in-differences empirical approach, they find that ABLs lead to modest decreases in the probability of reporting having been recently bullied, and reductions in the prevalence of physical forms of bullying—specifically engagement in physical altercations both on and off school property. Furthermore, using a panel on youth crime data composed from the FBI's Uniform Crime Reports, they also find that ABLs lead to a reduction in minor teen school shooting deaths and violent crime arrests.

TABLE 17.
Anitbullying Law Effective Dates and Components, by State

State	Effective Date	School District Deadline	Reporting	Investigations	Training	Written Records and Transparency
Alabama	7/2010	X	X	X	X	
Alaska	7/2007	X	X			
Arizona	7/2005		X	X		
Arkansas	7/2003		X		X	
California	7/2012		X	X	X	X
Connecticut	2/2009	X	X	X	X	X
Deleware	1/2008	X	X	X	X	
Florida	12/2008	X	X	X	X	X
Georgia	8/2011	X	X	X	X	
Hawaii	7/2011		X	X	X	
Idaho	7/2006					
Illinois	6/2010				X	
Indiana	7/2006		X	X	X	
Iowa	9/2007	X	X	X	X	
Kansas	7/2008				X	
Kentucky	12/2008	X	X	X	X	
Lousiana	8/2001	X				
Maine	9/2006	X			X	
Maryland	7/2009	X	X	X	X	X
Massachustes	1/2011	X	X	X	X	
Michigan	6/2012	X	X	X	X	
Minesotta	8/2007					
Mississippi	1/2011	X	X	X		
Missouri	9/2007	X	X		X	
Montana	10/2015					
Nebraska	7/2009	X				
Nevada	7/2005		X		X	X
New Hampshire	1/2011	X	X	X	X	X
New Jersey	9/2003	X	X	X	X	
New Mexico	4/2007	X	X	X	X	
New York	7/2013	X	X	X	X	X
North Carolina	1/2010	X	X	X	X	
North Dakota	7/2012	X	X	X	X	
Ohio	10/2010	X	X	X	X	X
Oklahoma	11/2008			x	X	
Oregon	1/2002		X	X	X	
Pennsylvania	1/2009	X	X		X	
Rhode Island	9/2004	X	X		X	
South Carolina	1/2007	X	X	X	X	
South Dakota	7/2012		X	X		
Tennesse	1/2006	X	X	X		
Texas	6/2011		X	X	X	
Utah	9/2012	X			X	
Vermont	2/2007	X	X	X	X	X
Virginia	7/2005				X	
Washington	8/2011	X	X	X		X
West Virginia	12/2001	X	X	X	X	
Wisconsin	9/2010	X	X	X		
Wyoming	1/2010	X	X	X	X	

Notes: Effective Date is the school district deadline date, for states missing a school district deadline we list the effective date of the legislation. Montanta did not pass an ABL during our study period 1993-2014.

This study is the first to exploit the arguably exogenous temporal variation in the passage of state-level ABLs to explore the impact of ABLs, and specific components of ABLs, on teen suicidality and substance use. This study is also the first to explore the heterogeneous response to ABLs across gender; the prevalence and nature of bullying behavior varies greatly across gender—Craig, 1998; Faris and Felmlee, 2011—and as such it is important to understand how females separate from males respond to the bullying interventions.

Data

Outcome Measures

YRBS Data. The outcome data for our primary analysis come from the national and state YRBS covering the period 1993-2013. Government agencies use the YRBS data to track trends in high school behaviors such as violence, substance abuse, physical activity, and sexual conduct. Previous studies such as Birkett et al. (2009), Litwiller and Brausch (2013), and Messias et al. (2014) have used these data to examine the link between bullying and suicidality, violence, and substance abuse.

The national YRBS is conducted biennially by the Center for Disease Control (CDC) and is a nationally representative sample of U.S. high school students.[6] For

[6]Although intended to be nationally representative, not all 50 states are included in any given wave of the national YRBS. Between the years 1997-2013, only 7 states contribute data to the national YRBS every year. See Table A4 for more information.

the purpose of this study, we obtained restricted-use versions of the national YRBS data so we can link respondents to their state of residence. The national YRBS data used for the purpose of this study covers the period 1997-2013.[7]

Save for a few exception, most states conducted their own YRBS somewhere between 1993-2013. Most states have given the CDC permission to release their data, the remaining states require data use requests be made directly to them. The state YRBS is also high school based, and conducted biennially. It is coordinated by the CDC and mirrors the content of the national YRBS.[8] Table A5 shows the number of observations, by year and state, in the state YRBS. Following previous studies, we combine the state and national; YRBS so as many law changes as possible can contribute to identification (Anderson et al., 2015; Hansen et al., 2015; Sabia and Anderson, 2016). In total, the YRBS data cover all 50 states.

For our analysis, we identify one measure of suicidality and one measure of drug use. First, we generate a measure of suicidality using the following 3 survey questions:

"During the past 12 months, did you ever seriously consider attempting suicide?"

"During the past 12 months, did you make a plan about how you would attempt suicide?"

[7]The national YRBS was first conducted in 1991. However, because the first three waves of the YRBS omit most questionnaire items of interest, we begin with the national YRBS data in 1997.

[8]The state YRBS is coordinated by the CDC, however unlike the national YRBS, it is administered by state education and health agencies

"During the past 12 months, how many time did you actually attempt suicide?"

Suicidality is coded equal to 1 if a respondent answered "yes" for either of the first 2 questions, or a positive value for the last question, and zero otherwise. In addition to the above coding, we also explored the differential effect of treatment on suicidal ideation versus attempted suicide but found no significant difference in the effect of an ABL on each measure of suicidality.

Next, we create a measure for *Drug Use*. Students were asked the following 2 questions:

"During your life, how many times have you used heroin (also called smack, junk, or China White)?"

"During your life, how many times have you used methamphetamines (also called speed, crystal, crank or ice)?"

Drug Use is set to 1 if the respondent answers at least once for either of the above, and 0 otherwise.[9] Of all the questionnaire items about drug use, these two were singled out because, heroin and methamphetamine use are most strongly correlated with bullying (Srabstein et al. 2008).[10] Note, we also explored the impact of ABLs on frequency of drug use, and attempted suicide, but this alternative analysis did not provide statistically significantly different results from our baseline analysis.

[9]We also explored the impact of ABLs on frequency of drug use and find no differential impacts.

[10]Perscription drug use is also highly correlated with bullying victimization, however the YRBS only asks about the consumption of prescription drugs in a small subset of years.

Table 18 provides descriptive statistics for the combined YRBS. In Table 18, we see that 20% of the sample has considered or attempted suicide and 6% of the sample has used heroin and/or methamphetamines in their lifetime. The average child in the sample is about 16 years old and about 49% of the sample is male. As we will focus our analysis on the impact of ABLs by gender, it is important to note about 16% of males in the sample have had suicidal thoughts or attempts; while 24% of females in the sample have considered or attempted suicide. With regards to drug use, males are more likely to have done heroin or methamphetamines in their lifetime; 7% and 5% of males and females respectively answered affirmatively to drug use.

Note, one pitfall of using the YRBS data is that it is self-reported in nature. If these ABLs induce students to be more willing to report bullying, and seek counseling, then these students may be more willing to admit to bullying, and even bullying-related outcomes, on a survey. This would lead us to underestimate the effects of ABLs. Additionally, large measurement error on the outcome measures could bias our standard errors upwards. Accordingly, we supplement our analysis with administrative data on teen deaths resulting from suicide or illegal drug overdose.

TABLE 18.
Descriptive Statistics, YRBS 1993-2013

	Variable Description	Mean (SD)
Suicidality	Considered or attempted suicide in the past year	0.204 (0.403)
Drug Use	Used meth, heroin, or illegal injection in lifetime	0.060 (0.238)
Age	Ages 12-18	15.98 (1.225)
Male	Student is male (as opposed to female)	0.486 (0.500)
Black	Student is Black/African American	0.149 (0.356)
White	Student is White	0.571 (0.495)
Other	Student is race other than white or black	0.280 (0.449)
9th Grade	Student is in the 9th grade	0.281 (0.449)
10th Grade	Student is in the 10th grade	0.267 (0.442)
11th Grade	Student is in the 11th grade	0.244 (0.430)
12th Grade	Student is in the 12th grade	0.207 (0.405)
Observations		955,666

Notes: The data source is the YRBS, 1993-2013.

Mortality Data. The mortality data are from the Multiple Cause-of-Death files published annually by the National Center for Health Statistics. Multiple Cause-of-Death data cover all 50 states from 1990-2014. The mortality data contain information on state of residence, month of death, age, and gender of the deceased. Between the years 1977-1998, the International Classification of

Diseases, 9th Edition (ICD-9) was used to code mortality; from the year 1999 to the present the ICD-10 is used. In the ICD-9, suicides are defined by code 350 in the "34 Recode" classification, and accidental drug overdoses are defined by code 317 in the "282 Recode" classification. In the ICD-10, suicides are defined by code 040 in the "39 Recode" classification, and accidental drug overdoses are defined by code 420 in the "358 Recode" classification. We look at monthly state-level suicide and drug overdoses for adolescents aged 14 to 18 years old.

We will explore how ABLs impact adolescent suicide and overdose rates; in order to get month-by-year suicide rate per 10,000 adolescent population, the number of suicides in a state in a given month of a year are multiplied by 10,000 and divided by the gender-age population of interest. The monthly drug overdose rate per 10,000 adolescent population is derived similarly. Note, completed suicide and illicit drug overdose are rare outcomes for this adolescent group—no state experiences more than 8 overdoses or 20 suicides in a given month.

Covariates

We obtain state-year violent and property crime rates from the FBI's *Uniform Crime Reports*, state-level demographics were calculated using population data from the National Cancer Institute's Surveillance Epidemiology and end Results Program. State-year Unemployment rate and Median Household Income were gathered from the Bureau of Labor Statistics and the US Census Bureau respectively. To control for state spending on schools, we obtain total

expenditure per pupil data from the National Center of Education Statistics. Last, all information pertaining to the effective dates, as well as the content, of the aforementioned legislations was collected using information from the U.S. DOE Office of Planning, Evaluation and Policy Development report (U.S. DOE, 2011), and independent investigation of state legislative history.

Empirical Strategy

To estimate the relationship between state ABLs and high school student outcomes, we exploit temporal variation in these state laws in a standard difference-in-differences framework. Specifically, we estimate the following equation via ordinary least squares:

$$Y_{ist} = \beta_0 + \beta_1 ABL_{st} + \beta_2 X_{ist} + \alpha_s + \gamma_t + \alpha_s \cdot t + \epsilon_{ist} \tag{4.1}$$

We begin our analysis using data from the 1993-2013 pooled YRBS; as such, the dependent variable Y_{ist}, represents our previously described binary measures of suicidality and drug use. The independent variable of interest, ABL_{st} is an indicator for whether state s was enforcing an ABL in year t.[11] In alternative specifications, ABL_{st} also represents a set of indicators measuring the specific ABL

[11] All laws are coded into effect in the year following the law; for example if a law passes in September 2009, we have the law coded into effect in 2010. Laws are coded this way, primarily because YRBS survey questions are retrospective. We predict that if this decision impacts our results it will attenuate them.

components of interest. The vectors α_s and γ_t represent state fixed effects and year fixed effects respectively. The vector X_{ist} represents individual-level controls including age, race, gender, and grade level; X_{ist} also represents state-level time varying controls including violent and property crime rates, adolescent population, racial composition, economic conditions, and total expenditure per pupil. We include state-specific linear time trends, $\alpha_s \times trend$, to control for unmeasured linear state trends in outcomes. The inclusion of state-specific time trends to allow us to interpret the parameter of interest, $\hat{\beta}_1$, as the average deviation from state-specific trends coincident with treatment. Last, we estimate the error term allowing for state-level clustering.

The main identifying assumption of our difference-in-differences approach is that suicidality and drug use for children in treatment states would have changed in a way similar to children in control states in the absence of the legislative change. After reporting baseline estimates, we will examine whether there is evidence of divergence between treatment and control states prior to treatment by estimating our primary specification including leading indicators.

Results

YRBS

To examine how the passage of a state antibullying statute impacts adolescent mental health and risky behavior outcomes, we consider the average

effect of passing an ABL on the probability of reported suicidal behavior or reported substance use. In all panels, in columns (1) and (2) we report estimates corresponding to the treatment effect on the probability of reported suicidality and drug use, respectively, for the total sample. Columns (3) and (4) report the same information from analysis conducted on the sample restricted to only males, and columns (5) and (6) the same information from analysis conducted on the sample restricted to only females.

The results we depict in Panel A of Table 19 are the estimated effects of ABLS on the probability of suicidal behavior and drug use controlling for state and year fixed effects. In Panel B, we add state-level time varying controls and individual-level controls. Last, in Panel C, our preferred specification, we additionally control for differences in state-specific trending of the mental health outcomes. The results presented in panels B and C suggest that ABLs are associated with a decrease in the probability of reported suicidal behavior and an increase in the probability of reported drug use; however, our results are not statistically distinguishable from zero.

TABLE 19.
Antibullying Laws (ABLs) on Suicidality and Illicit Drug Use, by Gender

	Total		Male		Female	
	Suicidality	Drug Use	Suicidality	Drug Use	Suicidality	Drug Use
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Fixed Effects						
ABL	-0.000	0.001	-0.001	0.000	0.000	0.002
	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)	(0.004)
Panel B: Fixed Effects+ Individual and State-Level Control						
ABL	-0.004	0.002	-0.004	0.000	-0.004	0.003
	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)
Panel C: Fixed Effects+ Individual and State-Level Control+ State Specific Linear Trends						
ABL	-0.003	0.000	-0.004	-0.001	-0.002	0.002
	(0.005)	(0.003)	(0.004)	(0.003)	(0.006)	(0.003)
Observations	943,957	863,906	460,590	420,419	483,367	443,487

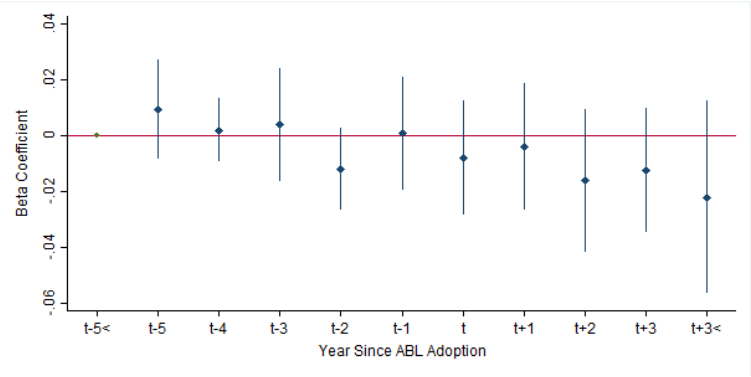
Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each column corresponds to estimates from a seperate OLS regression using data from the 1993-2013 YRBS. Each specification accounts for state and year fixed effects, state specific linear time trends, and covariates. The control variables include student race, gender, age, and grade, and state-level population and demographics, unemployment rate, median household income, and violent and property crime rates. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

Note that the estimated treatment associations reported in Table 19 could suffer from attenuation bias if states that adopt ABLs experience declining trends in suicidality and drug use. To test the concern that outcome trends are confounding the true effect of the legislation, we conduct an event study. We re-estimate Equation (1) allowing for different levels across all pre- and post-treatment years. Specifically, we add separate indicator variables for one to five years prior to treatment, the year of treatment, one to 3 years post-treatment, and 4 or more years post treatment—the omitted category is more than five years prior to treatment—these results are summarized in Figure 13. Our results provide little evidence to support the notion that differential trends are confounding the results summarized in Table 19 as indicated by the policy lead estimates which are close

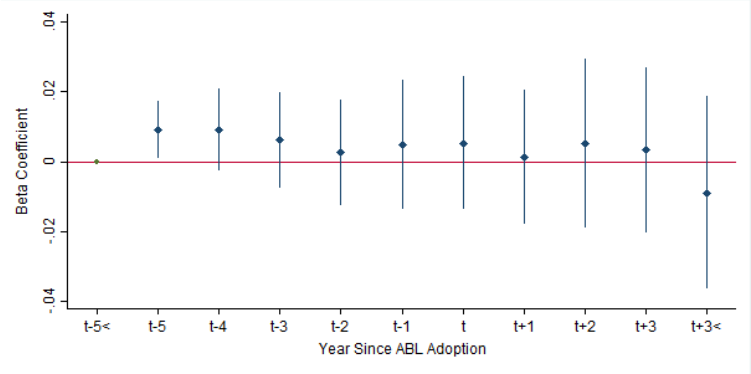
to zero. Furthermore, as indicated by estimates on the lag indicators, we see no evidence that over time these laws take effect.

FIGURE 13.
Antibullying Laws (ABLs) on Suicidality and Illicit Drug Use, Before and After Effective Legislation years

(a) ABL on suicidality



(b) ABL on illicit drug use



Notes: The figures display coefficient estimates, and their 95% confidence intervals for the leading indicators and lagged treatment, from an OLS regression, including state and year fixed effect, state-specific time trends, and covariates. The control variables include student race, gender, age, and grade, and state-level population and demographics, unemployment rate, median household income, and violent and property crime rates. Standard errors are adjusted for clustering at the state-level.

The results of our baseline analysis indicate that, on average, ABLs appear to have no effect on suicidality and drug use; however, as mentioned previously there is considerable heterogeneity in state ABLs, and thus there may be differential

effects across ABL components. The results from this component analysis on suicidality and drug use are presented in tables 20 and 21 respectively. In both tables, columns (1) through (3) correspond to male outcomes and columns (4) through (6) correspond to female outcomes. In Column (1), we present the baseline treatment effect estimate—same as Table 19, Panel C—in Column (2), I examine whether the effects of an ABL on sucidality are different when laws explicitly mandate a school district deadline. Our estimates indicate that a school district deadline component decreases the probability of suicidality, at the pre-treatment mean, by 8% and this estimate is significant at the ten-percent level. The effect of any ABL on suicidality is now positive, but remains insignificant. These results suggest that ABLs with a strict deadline for component implementation have a beneficial effect on bullying-related outcomes such as sucidality, but ABLs with no clear deadline may have a detrimental effect.

To further explore the efficacy of ABLs we look at the individual effect of each of the four major school district components—reporting, investigations, training, and written records/transparency—on suicidality (Column (3)). Estimates suggest that, among males, a reporting components significantly increases the probability of reporting suicidal behavior by about 7%; however, a written records/transparency component decreases th probability of reported suicidality among males by almost 11%. The corresponding estimates for females, presented

in columns (3) through (6), are similar to those reported for males, but these estimates are statistically indistinguishable from zero.

TABLE 20.
Antibullying Law (ABL) Components on Suicidality, by Gender

	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
ABL	-0.004	0.005		-0.002	0.007	
	(0.004)	(0.006)		(0.006)	(0.007)	
School District Deadline		-0.014*	-0.020***		-0.014	-0.019*
		(0.008)	(0.007)		(0.009)	(0.009)
Reporting			0.020**			0.010
			(0.010)			(0.017)
Investigations			-0.005			0.007
			(0.009)			(0.016)
Training			0.006			0.004
			(0.006)			(0.007)
Written Records and Transperancy			-0.021**			-0.022*
			(0.008)			(0.012)
Observations	460,590	460,590	460,590	483,367	483,367	483,367
Pre-Treatment Mean	0.171	0.171	0.171	0.255	0.255	0.255

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each column corresponds to estimates from a seperate OLS regression using data from the 1993-2013 YRBS. Each specification accounts for state and year fixed effects fixed effects, state specific linear time trends, and covariates. The control variables include student race, gender, age, and grade, and state-level population and demographics, unemployment rate, median household income, and violent and property crime rates. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

The analysis presented in Table 21 is analogous to that presented in Table 20, however the outcome of interest is the probability of reporting illicit drug use. Our results again estimate that a deadline component significantly decreases the probability of reported drug use among males with an impact of 12%. Furthermore, evidence suggests that an investigation component increases the probability of reporting drug use for both males and females; the estimate is significant at the five-percent level for females. This analysis lends further evidence that ABLs with strict deadlines are most effective. The analysis we present in tables 21 and 20 lend support for concern that since reporting and investigation systems are often

operated by the existing school guidance counselor, these two ABL components may displace this existing mental health resource.

TABLE 21.
Antibullying Law (ABL) Components on Suicidality, by Gender

	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
ABL	-0.001	0.005		0.002	-0.001	
	(0.003)	(0.003)		(0.003)	(0.003)	
School District Deadline		-0.009*	-0.008		-0.004	-0.006
		(0.005)	(0.005)		(0.005)	(0.006)
Reporting			0.000			0.016*
			(0.011)			(0.009)
Investigations			0.007			0.018**
			(0.012)			(0.009)
Training			-0.003			-0.004
			(0.003)			(0.004)
Written Records and Transperancy			-0.001			0.002
			(0.006)			(0.007)
Observations	420,419	420,419	420,419	443,487	443,487	443,487
Pre-Treatment Mean	0.072	0.072	0.072	0.052	0.052	0.052

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each column corresponds to estimates from a seperate OLS regression using data from the 1993-2013 YRBS. Each specification accounts for state and year fixed effects fixed effects, state specific linear time trends, and covariates. The control variables include student race, gender, age, and grade, and state-level population and demographics, unemployment rate, median household income, and violent and property crime rates. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

Mortality

We now continue our analysis of the impact of ABLs on risky behaviors and mental health using a state-by-month-by-year panel, for the period 1990-2014, from the individual death records recorded in the multiple cause-of-death data files. We re-estimate Equation (1) where our outcomes of interest are now the adolescent (ages 14-18) population rate of completed suicide and illicit drug overdose per 10,000 adolescents. Our preferred specification now controls for month-by-year fixed effects in addition to the state-specific linear trends. Our analysis for the effect of

ABL and ABL components on the rate of adolescent suicide and drug overdose is summarized in tables 22 and 23 respectively. These tables are analogous to tables 20 and 21, respectively, from the previous analysis utilizing YRBS data on reported suicidal and drug using behavior. As before, we find no average effect of ABLs on suicide or drug overdose rates (columns (1) and (4) of tables 22 and 23), but we find that a deadline component is negatively associated with reductions in both suicide and drug overdose rate. Note, this effect is only significant for the rate of male overdoses per 10,000; the impact of this effect is a 23% reduction. The results we present in Column (3) of Table 22 indicate that, as with reported suicidality, a reporting component significantly increases the male rate of suicide, and a written records/transparency component significantly decreases the male rate of suicide; however, the impacts of these components are greater for completed suicide than for reported suicidal behavior (19% and 18% versus 7% and 11% respectively). Unlike with reported drug use, the written records/transparency component also significantly decreases the female rate of illicit drug overdose (Column (6), Table 23).

TABLE 22.
Antibullying Law (ABL) Components on Suicide Rate per 10,000, by Gender

	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
ABL	0.024	0.018		-0.045	-0.041	
	(0.055)	(0.061)		(0.029)	(0.042)	
School District Deadline		0.010	0.001		-0.006	-0.004
		(0.089)	(0.103)		(0.047)	(0.050)
Reporting			-0.212**			-0.108
			(0.090)			(0.069)
Investigations			0.081			0.059
			(0.083)			(0.069)
Training			0.116			-0.014
			(0.075)			(0.042)
Written Records and Transperancy			0.210*			0.059
			(0.106)			(0.054)
Observations	14,400	14,400	14,400	14,400	14,400	14,400
Pre-Treatment Mean	1.171	1.171	1.171	0.309	0.309	0.309

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each column corresponds to estimates from a seperate OLS regression using data from the 1993-2013 YRBS. Each specification accounts for state and year fixed effects fixed effects, state specific linear time trends, and covariates. The control variables include student race, gender, age, and grade, demographics, unemployment rate, median household income, and violent and property crime rates. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

The results presented in tables 22 and 23 are consistent with those reported in tables 20 and 21; nonetheless, the components of an ABL generally have a greater impact, at the mean, for completed suicide and drug overdose rates versus reported behavior. This could indicate that ABLs matter more for students who are most effected by bullying and/or in the greatest need of mental health resources. This analysis lends further evidence for the argument that certain components—namely reporting and investigations—that displace existing mental health resources may exacerbate adolescent mental health issues. Furthermore, a

school accountability measure, such as a written recording system and transparency mandate, appears to most effectively reduce suicidal behavior and drug use.[12]

TABLE 23.
Antibullying Law (ABL) Components on Illicit Drug Overdose Rate per 10,000, by Gender

	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
ABL	-0.015	-0.002		-0.018*	-0.015	
	(0.028)	(0.025)		(0.009)	(0.011)	
School District Deadline		-0.022	-0.024		-0.006	-0.015
		(0.043)	(0.046)		(0.016)	(0.017)
Reporting			0.005			0.029
			(0.044)			(0.023)
Investigations			-0.014			-0.009
			(0.034)			(0.019)
Training			0.002			-0.022
			(0.039)			(0.017)
Written Records and Transperancy			0.023			-0.014
			(0.068)			(0.023)
Observations	14,400	14,400	14,400	14,400	14,400	14,400
Pre-Treatment Mean	0.097	0.097	0.097	0.039	0.039	0.039

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each column corresponds to estimates from a seperate OLS regression using data from the 1993-2013 YRBS. Each specification accounts for state and year fixed effects fixed effects, state specific linear time trends, and covariates. The control variables include student race, gender, age, and grade, demographics, unemployment rate, median household income, and violent and property crime rates. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

Conclusion

Although it has been over a decade since the enactment of the first ABL, little is known about how effective these laws are at mitigating the impacts of bullying outcomes for school-aged children, in particular, we lack a comprehensive understanding of the efficacy of different ABL components. All the while, bullying

[12]Note, although not presented in the paper, we also explore if there are heterogeneous effects of ABLs across months of the year, in particular to explore differential effects during the academic year versus summer vacation. We find no significant differences.

related outcomes such as teen substance use and suicide rates continue to trend upwards. This study aims to contribute to the limited body of work speaking to how state ABLs impact student outcomes. Specifically, using survey and administrative data, this study is the first to investigate the effect of ABLs on adolescent suicide behavior and substance use. Furthermore, focusing on the impact of heterogeneous ABL components lends insight on how to more effectively allocate resources to school-level bullying prevention programs.

Controlling for within-state trends in outcomes, we find modest evidence that ABLs, those with a strict policy deadline, significantly decrease adolescent male suicidality and overdose rates with an impact of 11% and 22% respectively. These results appear to be primarily driven by school accountability components. When we consider the effects of specific ABL components, our evidence indicates that, on average, an ABL that mandates a reporting and/or investigations component is associated with an increase in the probability of adolescent drug use, overdose, and suicidal behavior. These findings indicate that certain ABL components may put a strain on existing mental health resources in schools. Each ABL components comes at a cost, so as schools make decisions about which components to include and which to exclude from their ABLs, it is important that empirical evidence is available for guidance. For example, New Jersey is currently debating whether to relax its strict investigations component; the argument being posed is that the cost, in terms of displaced resources, outweighs the benefit (New Jersey

Spotlight, 2016). Also, Minnesota recently added a school staff training mandate, and strict reporting requirements to its ABL; enforcement of this new, and more comprehensive, ABL is estimated to cost school districts almost $20 million annually (Minnesota Management and Budget, 2013). Thus, this study aims to better inform policy facing such decisions.

Furthermore, this study suggests that more comprehensive analysis exploring the relationship between bullying, bullying outcomes, and the costs of antibullying laws are required in order to better assess the efficacy of antibullying interventions. In addition, a better understanding of the causal relationship between bullying and student mental health and risky behaviors is required to craft more holistic bullying legislation, while reducing potential unintended consequences. Last, studies must pay special attention to the effect of antibullying interventions on historically targeted subpopulations such as the LGBT community, immigrants, and younger children in order to ensure polices accommodate those most in need. More generally, this study highlights the need for higher quality administrative and self-reported data pertaining to bullying incidences.

CHAPTER V

CONCLUSION

Using applied econometric techniques, we provide insight on a number of public health and welfare policies regarding disadvantaged youth populations.

In Chapter II, we examine the relationship between legal representation for foster youth, in abuse and neglect hearings, on adoption outcomes. Using variation in the implementation of state-level statutes mandating all foster care children have the right to representation, this study is the first to provide causal evidence that foster child legal representation induces faster adoptions and lower foster care reentry rates from adoptive homes. In addition, we find that the impact of legal counsel is strongest for those subgroups who are most at-risk for long term foster care stays. Back-of-the-envelope calculations indicate that the appointment of legal counsel to foster care children in dependency proceedings is not prohibitively expensive as the net savings accrued from speedier adoptions outweigh the costs of attorney representation. Policy implications from this work are clear in suggesting that providing legal counsel for foster children who will be adopted yields financial and welfare benefits.

In the next chapter, we go on to demonstrate that the legalization of medical marijuana results in lower rates of child physical abuse reporting and perpetration. Specifically, we find that caretaker marijuana use decreases the rate of child

physical abuse by 9%, with no effect on the rate of child neglect. Differential effects suggest the impact of marijuana use on the perpetration of child physical abuse is primarily driven by adult males, and that these impacts are greatest for white caretakers, and those over the age of 25. Although our results can't speak directly to the effect of general marijuana use on the perpetration of child maltreatment, as many studies have shown that MMLs increase adult marijuana use among nonpatients, we anticipate that policies which further increase marijuana accessibility will lead to qualitatively similar child maltreatment outcomes. In addition, as there are currently 20 states without an MML in effect, our study speaks directly to the potential child welfare benefits of such a law.

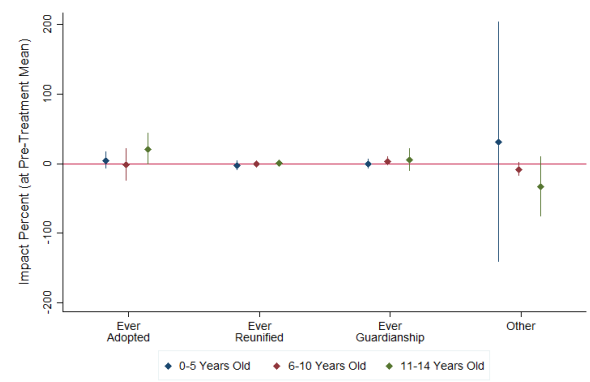
Finally, in Chapter IV, we find that, state-level antibullying laws (with a school district implementation date) result in modest reductions in adolescent suicidality and illicit drug use. However, our analysis reveals that certain time-intensive components of the law—such as a procedure for investigating reported bullying—may be displacing existing mental health resources in schools, as these procedural components are associated with increases in the probability of illicit drug use and suicide. Overall, the evidence suggests that through their effect on bullying behavior, antibullying laws have the potential to mitigate bullying related mental health outcomes; however, without additional financial support and resources, the implementation of certain procedural components may serve to worsen student health outcomes.

APPENDIX

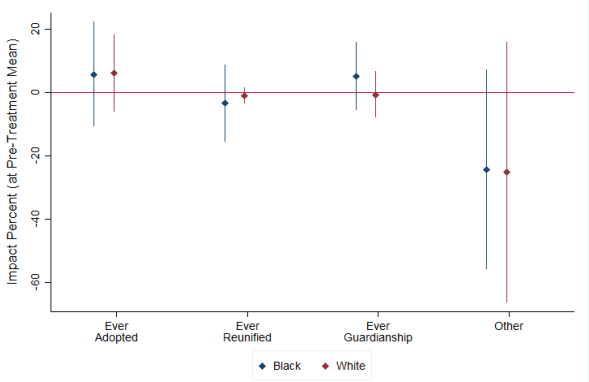
SUPPLEMENTAL FIGURES AND TABLES

FIGURE A1.
Lawyer Guardian Ad Litem (LGAL) on Discharge Type,
by Subgroup

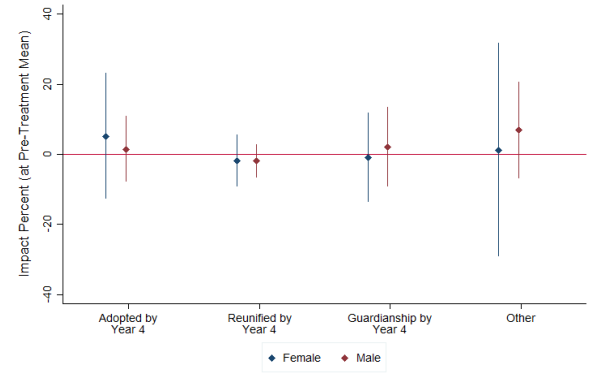
(a) By age, impact percent (at pre-treatment mean)



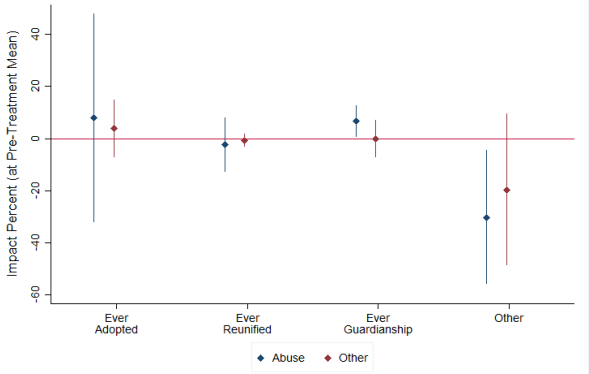
(b) By race, impact percent (at pre-treatment mean)



(c) By gender, impact percent (at pre-treatment mean)



(d) By reason for removal, impact percent (at pre-treatment mean)



Notes: The figure displays the marginal effects and their 95% confidence intervals from multinomial logit regressions, accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. Each panel corresponds to a separate regression. Full results from these regressions shown in tables A2 and A3. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level.

TABLE A1.
Lawyer Guardian Ad Litem (LGAL) Mandate on Adoption Within the First 2 Years, Before and After Effective Legislation years

	In Year 1 (1)	In Year 2 (2)
6 or More Years Prior	-0.006***	-0.016
	(0.002)	(0.011)
5 Years Prior	-0.004***	-0.001
	(0.002)	(0.009)
4 Years Prior	-0.004**	-0.006
	(0.002)	(0.010)
3 Years Prior	-0.005***	-0.013*
	(0.001)	(0.007)
2 Years Prior	-0.004***	-0.006
	(0.001)	(0.005)
Treatment Year	0.002	0.016***
	(0.001)	(0.004)
1 Year After	0.003	0.013*
	(0.002)	(0.008)
2 Years After	0.004	0.012
	(0.003)	(0.016)
3 Years After	0.002	0.023
	(0.004)	(0.014)
4 Years After	0.006	0.045*
	(0.008)	(0.025)
5 Years After	0.007	0.065*
	(0.009)	(0.034)
6 Years After	0.006	0.035**
	(0.005)	(0.016)
Observations	1,195,272	1,195,272
State and Month-by-Year Fixed Effects	Yes	Yes
State-Specific Linear Time Trends	Yes	Yes
Individual- and State-Level Controls	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each column represents leading indicators and lagged treatment effects corresponding to outcome categories 1 and 2 from a multinomial logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

TABLE A2.
Lawyer Guardian Ad Litem (LGAL) Mandate on Discharge Type, by Age

	Adoption (1)	Reunification (2)	Guardianship (3)	Other (4)
Panel A: 0-5 years old				
L-GAL Mandate	0.009	-0.015	-0.002	0.008
	(0.013)	(0.017)	(0.007)	(0.010)
Observations	745,234	745,234	745,234	745,234
Pre-Treatment Mean	0.276	0.480	0.143	0.041
Impact (%) at Pre-Treatment Mean	3.2	-3.1	-1.3	20.0
Effect Size	0.019	0.030	0.006	0.034
Panel B: 6-10 years old				
L-GAL Mandate	-0.006	-0.002	0.005	0.002
	(0.016)	(0.014)	(0.008)	(0.006)
Observations	283,144	283,144	283,144	283,144
Pre-Treatment Mean	0.121	0.583	0.135	0.086
Impact (%) at Pre-Treatment Mean	-4.6	-0.4	4.1	2.6
Effect Size	-0.017	-0.004	0.017	0.007
Panel C: 11-14 years old				
L-GAL Mandate	0.015*	0.004	0.011	-0.031
	(0.008)	(0.008)	(0.016)	(0.020)
Observations	166,894	166,894	166,894	166,894
Pre-Treatment Mean	0.072	0.635	0.201	0.092
Impact (%) at Pre-Treatment Mean	21.1	0.7	5.6	-33.2
Effect Size	0.061	0.009	0.029	0.094
State and Month-by-Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Linear Time Trends	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each row represents marginal effects from a multinomial logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

TABLE A3.
Lawyer Guardian Ad Litem (LGAL) Mandate on Discharge Type, by Subgroup

	Adoption (1)	Reunification (2)	Guardianship (3)	Other (4)
Panel A: Black				
L-GAL Mandate	0.014	-0.019	0.009	-0.004
	(0.013)	(0.030)	(0.011)	(0.014)
Observations	304,175	304,175	304,175	304,175
Pre-Treatment Mean	0.193	0.485	0.160	0.078
Impact (%) at Pre-Treatment Mean	7.3	-4.0	5.7	-5.2
Effect Size	0.036	0.038	0.027	0.013
Panel B: White				
L-GAL Mandate	0.006	-0.008	-0.004	0.006
	(0.011)	(0.008)	(0.007)	(0.004)
Observations	549,204	549,204	549,204	549,204
Pre-Treatment Mean	0.204	0.542	0.134	0.059
Impact (%) at Pre-Treatment Mean	2.9	-1.5	-2.7	9.7
Effect Size	0.014	-0.016	-0.011	0.023
Panel C: Female				
L-GAL Mandate	0.011	-0.010	-0.001	0.001
	(0.018)	(0.019)	(0.009)	(0.009)
Observations	581,872	581,872	581,872	581,872
Pre-Treatment Mean	0.205	0.523	0.145	0.062
Impact (%) at Pre-Treatment Mean	5.1	-1.9	-1.0	1.2
Effect Size	0.025	0.020	0.005	0.003
Panel D: Male				
L-GAL Mandate	0.003	-0.010	0.003	0.005
	(0.009)	(0.013)	(0.007)	(0.005)
Observations	613,400	613,400	613,400	613,400
Pre-Treatment Mean	0.203	0.532	0.131	0.069
Impact (%) at Pre-Treatment Mean	1.4	-1.9	2.0	6.8
Effect Size	0.007	-0.021	0.009	0.017
Panel E: Abuse				
L-GAL Mandate	0.013	-0.017	0.009	-0.005
	(0.028)	(0.033)	(0.006)	(0.008)
Observations	263,474	263,474	263,474	263,474
Pre-Treatment Mean	0.146	0.611	0.105	0.075
Impact (%) at Pre-Treatment Mean	8.9	-2.8	8.2	-6.3
Effect Size	0.035	0.034	0.028	0.017
Panel F: Other reason for removal				
L-GAL Mandate	0.004	-0.007	-0.003	0.005
	(0.010)	(0.009)	(0.008)	(0.006)
s Observations	887,416	887,416	887,416	887,416
Pre-Treatment Mean	0.219	0.506	0.146	0.063
Impact (%) at Pre-Treatment Mean	2.0	-1.3	-1.8	7.6
Effect Size	0.010	-0.013	-0.008	0.018
State and Month-by-Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Linear Time Trends	Yes	Yes	Yes	Yes
Individual- and State-Level Controls	Yes	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Each row represents marginal effects from a multinomial logit regression accounting for state, month, and year fixed effects, state specific linear time trends, and covariates. The control variables include race, gender, age at removal, and at the state level, population, percent black, percent white, unemployment rate, median household income, and TANF total expenditure. Standard errors are adjusted for clustering at the state-level and are shown in parentheses.

TABLE A4.
Number of Observations by State-Year, National YRBS

	1997	**1999**	**2001**	**2003**	**2005**	**2007**	**2009**	**2011**	**2013**	**Total**
AL	797	59	312	657	–	484	1,070	318	327	4,024
AR	362	–	–	282	–	417	299	–	308	1,668
AZ	1,108	131	408	346	285	617	361	1,132	178	4,566
CA	1,986	2,482	2,199	1,732	1,554	2,111	2,806	1,882	2,464	19,216
CO	268	–	660	–	–	–	193	297	305	1,723
CT	223	–	–	–	234	–	–	–	70	527
DE	–	–	–	366	–	–	–	232	–	598
FL	675	863	1,068	1,513	536	744	226	1,430	1,005	8,060
GA	344	812	486	424	1,852	349	1,319	136	371	6,093
HI	–	311	–	–	–	–	236	–	–	547
IA	784	–	–	–	238	247	–	–	–	1,269
ID	–	–	156	–	241	–	–	262	252	911
IL	–	230	440	317	494	594	1,500	993	650	5,218
IN	–	–	178	422	170	401	–	277	825	2,273
KS	204	–	–	340	277	–	199	301	194	1,515
KY	–	–	–	–	536	359	–	214	670	1,779
LA	580	624	–	692	158	–	434	–	–	2,488
MA	1,636	–	255	213	257	718	–	289	–	3,368
MD	823	–	–	261	–	–	–	–	529	1,613
ME	238	198	205	197	–	–	–	–	–	838
MI	523	525	341	398	307	297	320	628	490	3,829
MN	–	–	–	–	95	–	188	–	296	579
MO	–	556	464	265	102	345	85	345	269	2,431
MS	330	640	344	–	–	360	–	95	585	2,354
MT	–	–	186	–	–	–	–	–	–	186
NC	339	509	669	–	645	581	–	1,111	380	4,234
NJ	739	235	222	306	319	689	479	113	367	3,469
NM	280	–	155	104	–	221	606	–	–	1,366
NV	–	–	236	–	–	–	388	207	–	831
NY	358	729	309	913	482	918	1,199	645	383	5,936
OH	556	565	225	299	279	–	–	–	158	2,082
OK	223	–	397	–	235	280	–	–	–	1,135
OR	–	–	184	–	268	–	247	–	–	699
PA	272	487	–	316	424	210	1,068	450	264	3,491
RI	–	75	–	–	–	–	–	–	–	75
SC	527	800	–	887	286	–	–	–	–	2,500
SD	–	–	–	297	–	–	–	–	–	297
TN	578	265	611	–	395	163	–	291	–	2,303
TX	949	2,715	2,051	2,632	1,725	1,587	1,331	1,787	393	15,170
UT	–	–	–	178	274	199	–	–	–	651
VA	–	750	–	245	350	439	98	203	1,138	3,223
VT	–	–	–	256	–	–	–	–	–	256
WA	108	–	54	–	101	–	246	167	195	871
WI	293	541	236	179	241	178	683	656	–	3,007
WV	–	–	262	–	232	245	468	258	–	1,465
Total	16,103	15,102	13,313	15,037	13,592	13,753	16,049	14,719	13,066	130,734

Notes: Not all states participate in the national YRBS

TABLE A5.
Number of Observations by State-Year, State YRBS

	1993	1995	1997	1999	2001	2003	2005	2007	2009	2011	2013	Total
AK	–	1,535	–	–	–	1,439	–	1,268	1,218	1,279	1,183	7,922
AL	4,292	3,796	3,589	2,040	1,551	1,064	1,026	–	1,459	1,336	1,518	21,671
AR	–	2,207	1,929	1,435	1,670	–	1,503	1,563	1,628	1,327	1,494	14,756
AZ	–	–	–	–	–	3,369	3,217	2,931	2,485	2,747	1,566	16,315
CT	–	–	1,588	–	–	–	2,208	1,997	2,319	2,000	2,307	12,419
FL	–	–	–	–	4,161	4,002	4,479	4,371	5,385	6,006	5,863	34,267
GA	1,551	–	–	–	–	2,046	1,730	2,395	1,827	1,898	1,907	13,354
HI	1,155	931	1,268	1,220	–	–	1,627	1,148	1,456	4,172	4,467	17,444
ID	3,862	–	–	–	1,687	1,702	1,426	1,384	2,102	1,659	1,838	15,660
IL	3,894	2,964	–	–	–	–	–	2,362	2,934	3,509	3,149	18,812
KS	–	–	–	–	–	–	1,633	1,692	1,998	1,833	1,895	9,051
KY	–	–	1,438	–	–	1,563	3,232	3,483	1,726	1,759	1,587	14,788
LA	–	–	5,353	–	–	–	–	1,299	1,004	1,115	1,063	9,834
MD	–	–	–	–	–	–	1,398	1,486	1,590	2,793	51,241	58,508
ME	–	1,341	1,770	–	1,320	1,635	1,325	1,267	8,445	9,079	8,343	34,525
MI	–	–	3,685	2,610	3,523	3,374	3,172	3,426	3,316	4,083	4,138	31,327
MO	–	4,717	1,415	1,619	1,632	1,533	1,861	1,520	1,596	–	1,557	17,450
MS	1,374	1,253	1,467	1,538	1,790	1,471	–	1,563	1,763	1,751	1,559	15,529
MT	2,434	2,422	2,319	2,820	2,624	2,678	2,987	3,846	1,785	4,022	4,745	32,682
NC	2,376	1,717	–	–	2,522	2,522	3,822	3,397	5,550	2,216	1,791	25,913
ND	–	1,489	–	1,789	1,580	1,649	1,710	1,722	1,767	1,863	1,919	15,488
NE	3,118	–	–	–	–	2,913	3,706	–	–	3,719	1,824	15,280
NH	2,603	2,023	–	–	–	1,298	1,249	1,581	1,450	1,359	1,590	13,153
NJ	–	–	–	–	2,102	–	1,482	–	1,724	1,617	1,661	8,586
NM	–	–	–	–	–	–	5,417	2,560	4,890	5,685	5,325	23,877
NV	1,923	1,441	1,378	1,652	1,440	1,947	1,529	1,729	2,017	–	2,069	17,125
NY	–	–	3,415	3,269	–	9,078	9,457	12,771	14,137	12,517	10,026	74,670
OK	–	–	–	–	–	1,366	1,688	2,562	1,397	1,136	1,465	9,614
RI	–	–	1,398	–	1,361	1,775	2,316	2,133	3,106	3,814	2,357	18,260
SC	4,140	5,333	5,330	4,514	–	–	1,281	1,206	1,070	1,437	1,553	25,864
TN	3,179	–	–	–	–	1,919	1,529	2,019	2,176	2,583	1,847	15,252
TX	–	–	–	–	6,974	–	4,098	3,320	3,435	4,055	3,086	24,968
UT	4,218	3,067	1,305	1,479	1,042	1,360	1,437	1,898	1,544	1,657	2,118	21,125
VA	–	–	–	–	–	–	–	–	–	1,400	6,641	8,041
VT	–	–	–	6,868	6,967	5,928	6,997	5,744	8,190	8,267	–	48,961
WI	3,212	–	1,292	1,315	2,091	2,100	2,352	2,056	2,392	2,959	2,776	22,545
WV	2,766	2,046	1,772	1,308	–	1,719	1,317	1,353	1,603	2,119	1,753	17,756
WY	–	1,614	1,951	1,590	2,684	1,507	2,455	2,174	2,802	2,439	2,924	22,140
Total	46,097	39,896	43,662	37,066	48,721	62,957	86,666	87,226	105,286	113,210	154,145	824,932

Notes: Not all states participate in the national YRBS

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